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THE THEORY OF THE GAS ENGINE.

By DUGALD CLERK.

From Proceedings of the Institution of Civil Engineers.

I.

THE practical problem of the conversion of heat into mechanical work has long occupied the minds of engineers and scientists; the steam engine is a partial solution, but although perfect as a machine, its efficiency is so low that it can hardly be considered as satisfactory and final. As the result of the best modern practice it may be taken that the steam engine does not convert more than 10 per cent. of the heat used by it into work, and this in engines of considerable size and with boilers and furnaces fairly efficient. In small engines it is much less, indeed it is certain that few among the thousands of steam engines in daily use below 6 HP. give an efficiency greater than 4 per cent. The great cause of loss is the amount of heat necessary to change the water from the liquid to the gaseous state, most of this heat being rejected with the exhaust either into the condenser or the atmosphere. Many attempts have been made to use liquids of lower specific heat than water, and requiring less heat for evaporation, the principal being alcohol, ether and carbon bisulphide, but for obvious reasons no success has been attained.

To heated air as a means of obtaining power, the objection of loss by latent heat does not apply, the air is already in the gaseous state, and any heat added at constant volume increases the temperature, and therefore the pressure, without the complication of change of physical state. A high efficiency would therefore be expected, and according to Professor Rankine the efficiency of the fluid in the engines of the "Ericsson" was about 0.26; the efficiency of the furnace was however low, and accordingly the actual efficiency of the engine was no higher than that of the best steam engines now in use. In the "Stirling" hot-air engine, he found the efficiency of the fluid to be 0.3 with a higher efficiency of furnace than in Ericsson's.

In the Ericsson engine the air was heated at constant pressure, the volume augmenting and the power being given by the increased volume of the air as it entered the motor cylinder from the reservoir into which it had been compressed. The mean effective pressure was only 2.12 lbs. on the square inch; the size and friction of the engine for a given power was enormous. In the Stirling engine the air was heated at constant volume with increase in pressure, the power being obtained by subsequent expansion; the mean available pressure

was 37 lbs. per square inch, and the friction of the engine only amounted to one-tenth of the total indicated power. Both engines used the now well known contrivance, the regenerator, which was the invention of Dr. Stirling, and which is the cause in both of the high efficiency.

The failure of these engines was due to the rapid burning out of the cylinder bottoms by the direct action of the fire, it being found impossible to heat the air rapidly enough to the required temperature without maintaining the temperature of the metal surfaces much higher than the maximum temperature to be attained by the air. To overcome this slow heating of the air when in mass has been the object of many inventors, and a type has often been proposed with a closed furnace, and the air forced through this furnace keeping up the combustion, the hot products going to the motor cylinder and there doing work. This method of internal heating, however, introduces difficulties as grave as exist in the external method. The hot gases having to pass through pipes and valves to the motor cylinder renders it impossible to maintain a very high temperature without damage to the machine. Sir George Cayley was the first to make and work experimentally an engine of this type.

In view of these futile attempts, until very recently hot air was considered as among the failures of the past, and it was believed that, imperfect as the steam engine is, nothing was likely to succeed in producing a better result.

The great progress made in recent years with the gas engine, and its advance from the state of an interesting but troublesome toy to a practical powerful rival of the steam engine, has shown that air may after all be the chief motive power of the future. In the gas engine chemical considerations greatly modify the theory and prevent it from ranking as a simple hot-air engine; but to be thoroughly understood it is better first to consider the power to be obtained from air under certain theoretical conditions.

Three well defined types of engines have been proposed—

(1.) An engine drawing into its cylinder gas and air at atmospheric pressure for a portion of its stroke, cutting off communication with the outer atmosphere, and immediately igniting the mixture, the piston being pushed forward by the pressure of the ignited gases during the remainder of its stroke. The in-stroke then discharges the products of combustion.

(2.) An engine in which a mixture of gas and air is drawn into a pump, and is discharged by the return stroke into a reservoir in a state of compression. From the reservoir the mixture enters into a cylinder, being ignited as it enters, without rise in pressure, but simply increased in volume, and following the piston as it moves forward, the return stroke discharges the products of combustion.

(3.) An engine in which a mixture of gas and air is compressed or introduced under compression into a cylinder, or space at the end of a cylinder, and then ignited while the volume remains constant and the pressure rises. Under this pressure the piston moves forward, and the return stroke discharges the exhaust.

Several minor types have been proposed and many modifications of these three methods are used. A thorough understanding of these, however, renders it possible to judge the merits of any other.

Types 1 and 3 are explosion engines, the volume of the mixture remaining constant while the pressure increases. Type 2 is a gradual combustion engine in which the pressure is constant but the volume increases.

The author, in the course of his experiments on gas engines, has found that 1,537° Centigrade is the temperature usually attained by the ignited gases in his engine, and he has accordingly investigated the behaviour of air under different conditions at this temperature.

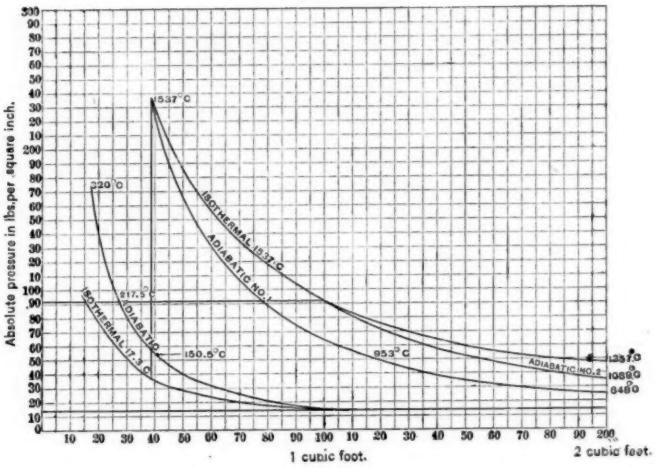
Type 1. Suppose an engine to have a piston with an area of 144 square inches and a stroke of 2 feet. Let the piston move through the first half of its stroke drawing into the cylinder air; let enough heat be immediately added to this air to cause it to rise instantly to 1,537° Centigrade, and the piston continue moving forward under the pressure produced. If there be no loss of heat through the sides of the cold cylinder, but the temperature of the air fall only through performing work, how much work would be done when the piston completes its out-stroke?

The air before the heat is added is supposed to be at a temperature of 17° Centigrade (about 60° Fahrenheit), and the ordinary atmospheric pressure. In Fig. 1 the line marked adiabatic No. 2 is the curve showing the work which would be obtained under the supposed conditions. Fig. 2. is the indicator diagram such an engine would furnish. It is not necessary here to detail the calculations. With this paper is given a table of the data used, so that the numbers may be verified. The following are the results:

Mean pressure during available part of stroke	39.8 lbs.
Temperature of air at the end of stroke	1,089° C.
Work done on piston	5,731 foot-lbs.

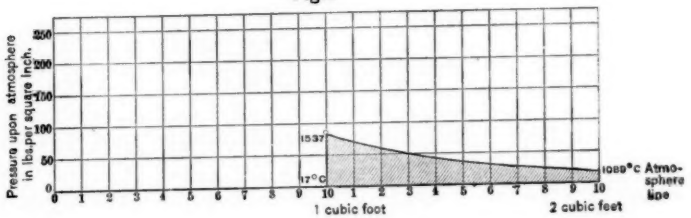
Duty of engine $\frac{5,731}{26,762} = 0.21$.

Fig.1.



As the engine is supposed to draw in air for half of its stroke, the last half of the stroke only is utilized for power; the mean available pressure calculated for the whole stroke is only $\frac{39.8}{2} = 19.9$ lbs.

Fig.2



1 cubic foot of air (at 170° Centigrade, and 760 millimetres mercury) remaining at constant volume requires to heat it to 1,537° Centigrade, an amount of heat equivalent to	26,762 foot-lbs.
Maximum pressure in lbs. per square inch above atmosphere	76.6 lbs.
Pressure at the end of stroke per square inch above atmosphere	19.6 lbs.

per square inch. There is a considerable pressure at the end of the stroke which could be made to give more work by expanding further; but for the purpose of comparison it is better to consider the three types of engine as each having a cylinder capacity swept by the piston of 2 cubic feet, and in each case using in its

operation 1 cubic foot of air at each stroke.

Type 2. Suppose an engine to draw into a pump 1 cubic foot of air, on its return stroke forcing the air into a reservoir at a pressure of 76.6 lbs. per square inch above the atmosphere. The motor piston is now at the beginning of its out-stroke, and as it moves forward air from the reservoir enters the cylinder, but as it enters it is heated to 1,537° Centigrade, without rise in pressure; the motor piston sweeps through 2 cubic feet.

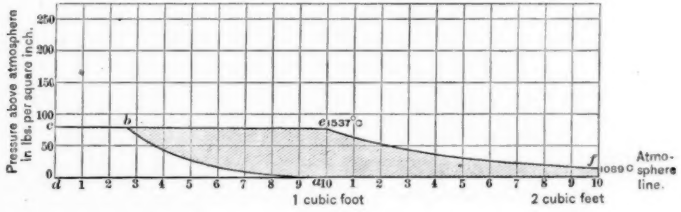
Fig. 3 shows the indicated card of this engine. *a b c d* is the pump diagram. Air at 17° Centigrade is taken in, compressed without loss of heat, the temperature rising under the compression to 217°.5 Centigrade. When it is equal to the pressure in the reservoir it is forced into the reservoir, as is shown on the line *b c*.

1 cubic foot of air (17° Centigrade and 760 millimeters mercury) at constant pressure requires to heat it from the temperature of compression 217°.5 to 1,537° Centigrade heat equivalent to.....	32,723 foot-lbs.
Maximum pressure in lbs. per square inch above atmosphere.....	76.6 lbs.
Pressure at end of stroke above atmosphere.....	19.6 lbs.
Mean pressure during available part of stroke.....	47.1 lbs. per square inch.
Temperature of air at the end of stroke.....	1,089° Centigrade
Work done on piston.....	11,759 foot-lbs.

$$\text{Duty of engine } \frac{11,759}{32,723} = 0.36.$$

Type 3. Suppose an engine to draw into a pump 1 cubic foot of air, on its return stroke forcing it into a reservoir at a pressure of 40 lbs. above the atmosphere. The motor piston is now at the beginning of its out-stroke, and as it moves forward air from the reservoir enters the cylinder while the piston sweeps through 0.39 cubic feet. At this point communication is cut off, and the temperature suddenly raised to 1,537° Centigrade. Hitherto the air has remained at the temperature of compression 150°.5. The pressure goes straight up to 220 lbs. above the atmosphere. This is shown at Fig. 1, and also at Fig. 4, which is the diagram of this type of engine, *a b c d* is the compression diagram; *a b e f* the motor diagram. The piston continues to move forward, and the air expands doing work. At the end of the stroke the pressure has fallen to 8.4 lbs. per square inch above the atmosphere.

Fig. 3



In all the operations no loss or gain of heat is assumed, except in doing work or in work being done on the air. In the motor diagram from c to e the air is flowing from the reservoir following the piston, and the temperature is 1,537° Centigrade during the whole admission. At *e* the communication with the reservoir is cut off, and the temperature falls while the air is expanding doing work, until it reaches the end of the stroke, when the exhaust is discharged by the return stroke of the piston.

For convenience the pump diagram is shown on the motor one, and the shaded portion represents the work done by the air as the result of the cycle. As the heat is added while the air expands in volume, it takes considerably more to raise a cubic foot of air to the required temperature than in the case of type 1.

1 cubic foot of sir (17° Centigrade, and 760 millimeters mercury) at constant volume requires to heat it from the temperature of compression 150°.5 Centigrade to 1,537° Centigrade heat, equivalent to....	24,416 foot-lbs.

Maximum pressure in lbs. per square inch above atmosphere	220 lbs.
Pressure at end of stroke	8.4 lbs.
Mean pressure during available part of stroke	47.8 lbs. per square inch.
Temperature at middle of stroke	953° Centigrade.
Temperature at end of stroke	648° Centigrade
Work done on the piston	11,090 foot-lbs

Duty of engine $\frac{11,090}{24,416}=0.45$.

The relative work obtained from 1 cubic foot of air heated to the assumed temperature is shown below.

RESULTS FROM ENGINES OF EQUAL VOLUME SWEPT BY MOTOR PISTON.

Type				
1.	5,731 foot-lbs.	work obtained	0.21	duty.
2.	11,759	" " "	0 36	"
3.	11,090	" " "	0.45	"

Fig.4

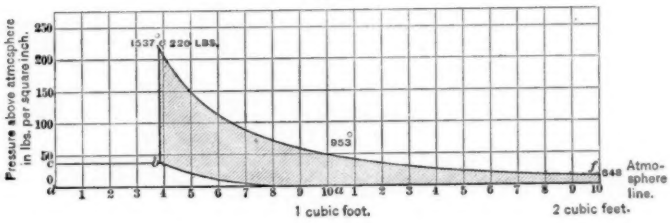
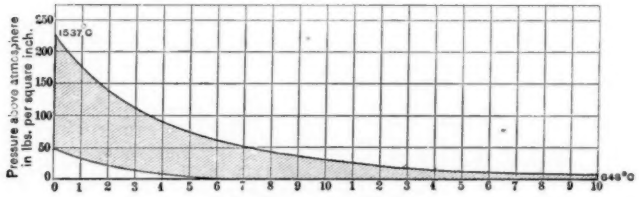


Fig. 5 shows the most important modification of this type; in it, instead of a separate reservoir, a space is left at the end of the cylinder, into which the piston does not enter, and in this space is compressed the gases forming the inflammable mixture. The rise in pressure therefore commences at the beginning of the stroke instead of when the piston has traveled out. In this diagram the volume swept by the piston and the clearance space together are supposed to be equal to 2 cubic feet. Comparing the results obtained from these three modes under precisely similar conditions, the same weight of air heated to the same degree, and used in cylinders of identical capacity, there is a considerable difference in the results possible even under the purely theoretic conditions stated.

That is, in an engine of type 1, if 100 heat-units be used, 21 units will be converted into mechanical work. In type 2, with the same amount of heat, 36 units will be given as work, and in type 3 no less than 45 units would be converted into work.

The great advantage of compression over no compression is clearly seen, by the simple operation of compressing before heating; the last type of engine gives for the same expenditure of heat 2.1 times as much work as the first. Compression, as used by the second type, does not afford so favorable a result; but even then the advantage is apparent, 1.6 times the effect being produced. By a greater degree of compression before heating even better results are possible. In an engine of type 3

Fig.5



PARTS OF THE STROKE.

expanding to the same volume after ignition as before compression, the possible duty D is determined by the atmospheric absolute temperature T′, and the absolute temperature after compression T; it is $D=\frac{T-T'}{T}$ whatever may be the maximum temperature after ignition. Increasing the temperature of ignition increases the power of the engine, but does not cause the conversion of a greater proportion of heat into work. With any given maximum temperature the smaller the difference between that temperature and the temperature of compression, the greater is the proportion of added heat converted into work with any given amount of expansion. The greater the compression before ignition, the more closely the two temperatures come together, and the higher is the duty of the engine; neglecting in the meantime the practical conditions of loss. What compression does is to enable a great fall of temperature to be obtained due to work done with but a small movement of the piston. In type 1 when the piston has reached the end of its stroke, the increase from the moment of ignition is only from one volume to two volumes, while in type 3 with the same total volume swept by the piston, it increases from one volume to five volumes. In the one case the ratio of expansion is two, while in the other it is five. This will be readily seen in Figs. 2 and 4. Now this increased expansion is not obtained at the cost of loss average pressure; in type 1 the mean available pressure over the whole stroke is nearly 20 lbs. per square inch, while in type 3 it is 38.5 lbs. per square inch; that is, the compression engine for equal size and piston speed has nearly twice the power of the other.

In the compression engine with a maximum temperature of 1,537° Centigrade, the final temperature is 648° Centigrade, while in the other, with the same maximum temperature, the final temperature is 1,089° Centigrade. It is true that by expanding sufficiently the same final temperature can be obtained without compression, but the average pressure will be low, and consequently less available for the production of power. To produce anything like an expansion of five times without compression the pressure would fall below the atmosphere, and it would be necessary to expand into a partial vacuum, and use a condenser and vacuum pump, as is done in the steam engine. Compression makes it possible to obtain from heated air a great amount of work with but a small movement of piston, the smaller volume giving greater pressures, and thus rendering the power developed more mechanically available. The higher the maximum temperature the greater the amount of compression which can be used advantageously. There is a degree of compression for every temperature, beyond which any increase causes a diminution of the power of the engine for a given size.

The compression in the author's engine is 40 lbs. per square inch above the atmosphere, and he has accordingly confined himself to the comparison of engines employing this amount of compression with those using no compression. Now, seeing that this difference is produced between engines of types 1 and 3 by the simple difference of cycle, when there is no loss of heat through the sides of the cylinder, the question arises which engine would give the greatest effect, which engine in actual practice, with a cylinder kept cold by water, would come nearest to theory? In which of the engines would there be the smaller loss of heat?

The amount of heat lost by a gas in contact with its enclosing cold surfaces depends, first, on the difference in temperature between the gas and the cooling surfaces; secondly, on the extent of surface exposed; and, thirdly, on the time of exposure. It would be very difficult to make an accurate numercial comparison between the engines, but all to be shown is, that in the one the loss of heat must be less than in the other.

To compare the two engines, take equal movements of the pistons from a maximum temperature of 1,537° Centigrade. In the engine working without compression this temperature is attained at the middle of its stroke, when the piston has moved through 1 cubic foot; the average temperature, while it moves to the end of its stroke, is about 1,300° Centigrade.

Now, in the compression engine the maximum temperature is attained at a

point when the piston has moved through 0.39 cubic foot: suppose it to move to 1.39 cubic foot, it has moved through 1 foot in the same time as the first engine. Then, as the temperature at the middle of the stroke is 953° (Fig. 4) it follows that the average during this movement is not higher than 1,000° Centigrade, but the space containing the heated air has increased from 0.39 cubic foot to 1.39 cubic foot, and with it the cooling surface; whereas the space containing heated air in the first engine has, during the same amount of movement, increased from 1 cubic foot to 2 cubic feet. It follows that as the temperature in the compression engine is 1,000° Centigrade during the same time as the temperature in the first engine is 1,300° Centigrade, and as the surface in it for cooling is also less, the amount of heat lost by the air must be less in the portion of the stroke under consideration. During the portion of the stroke remaining, 0.61 cubic foot, the temperature of the heated air is low, falling to 648° Centigrade at the end of the stroke; it follows that very small comparative loss results. Altogether the loss of heat by the compression engine will be the least.

It will be seen from Fig. 1 that there is a further cause of advantage. While the pressure and temperature are falling on adiabatic line 1, the work done by 1 cubic foot of air on expanding to the middle of the stroke at a temperature of 953° Centigrade is 7,888 foot-pounds, from 953° Centigrade to 648° is 3,202 foot-pounds, that is, 7,888 foot-pounds of work are performed by the engine during a movement of the piston equal to 0.61, while in the engine without compression a movement of 1.00 cubic foot only does 5,731 foot-pounds.

The compression engine during this portion of its stroke has converted the heat entrusted to it into work at twice the rate of the other engine. This is a great point. Any method which converts the heat into work with the utmost possible rapidity, by reducing the time of contact between the hot gases and the cylinder, saves heat and enables the theory of the engine to be more nearly realized.

Taking all circumstances into consideration, it is certainly not over estimating the relative advantage of the compression engine to say that it will, under practical conditions give, for a certain amount of heat, three times the work it is possible to get from the engine using no compression.

It will not be necessary to discuss the theory of type 2 in respect of loss of heat to the sides of the cylinder, as it is not much used, and has hitherto failed to yield results in any way equal to type 3. It will be seen, however, from Fig. 3, that the conditions are not so favorable for a minimum loss of heat as in type 3.

The temperature from the moment of admission at c, to the point of cut-off at e, is kept constant at 1,537° Centigrade, so that the loss of heat must be great, both the surface exposed and the mean temperature being high. It is the less necessary to discuss this point in the slow combustion engine, as the possibility of using a hot cylinder and piston reduces the loss by attaining a temperature not far removed from the entering air.

It will be interesting to calculate the amounts of gas required by these three types under the supposed conditions, and for this purpose an analysis of Manchester gas, and also of London gas, has been used as the basis of calculation.

ANALYSIS OF MANCHESTER COAL GAS.
BY BUNSEN AND ROSCOE.

Hydrogen	45.58
March gas	34.90
Carbonic oxide	6.64
Olefiant gas or ethylene	4.08
Tetrylene	2.38
Sulphuretted hydrogen	0.29
Nitrogen	2.46
Carbonic acid	3.67
	100.00 volumes.

Of this gas 1 lb. at atmospheric pressure and 17° Centigrade measures 30 cubic feet, and evolves on complete combustion 10,900 heat-units Centigrade, equivalent to 15,146,640 foot-lbs. 1 cubic foot of this gas will therefore evolve on complete combustion heat equivalent to $\frac{15,146,640}{30} = 504,888$ foot-lbs.

To obtain an idea of the difference in heating power of the different gases, there is given here a recent analysis of London gas.

ANALYSIS OF LONDON COAL GAS.

	(A.)	(B.)
Hydrogen	50.05	51.24
March gas	32.87	35.28
Carbonic oxide	12.89	7.40
Olefines	3.87	3.56
Nitrogen	—	2.24
Carbonic acid	0.32	0.38

Taking the average of the two analyses, 1 lb. weight of this gas at atmospheric pressure and 17° Centigrade, measures 35.5 cubic feet, and evolves on complete combustion 12,500 heat-units Centigrade, equivalent to 17,370,000 foot-lbs., 1 cubic foot of this gas will therefore evolve, on complete combustion, heat equivalent to $\frac{17,370,000}{35.5}=489,268$ foot-lbs.

The difference between the heat evolved by these gases is but small. As Glasgow coal gas is of a high illuminating power, it will be richer in olefines, and the heat evolved per cubic foot will be somewhat greater. Taking 505,000 foot-lbs. as the amount of heat evolved by 1 cubic foot of coal gas, the result is probably very near the average to be obtained from the coal gas of most towns. The number of foot-lbs. required for 1 HP. for one hour are $33,000\times60=1,980,000$. It therefore follows that if the whole heat to be obtained from gas were converted into mechanical work, 1 HP. for one hour requires $\frac{1,980,000}{505,000}=3.92$ cubic feet.

Now, taking the three types of engines, the amount of gas required by each to give 1 IHP. per hour would be as follows:

AMOUNT OF GAS REQUIRED BY THREE TYPES OF ENGINE.

Type 1. $\frac{3.92}{0.21}=18.3$ cubic ft. per HP. per hr.

" 2. $\frac{3.92}{0.36}=10.9$ " " "

" 3. $\frac{3.92}{0.45}=8.6$ " " "

If these engines be worked without loss of heat through the sides of the cylinders, but the expanding gases falling in temperature only through doing work, the above results would be obtained.

It is interesting to compare the consumption of gas by the engines in actual practice, to see in what order it stands. Results have not been obtained from engines of equal volume swept through by the piston, but it is at once seen that the order is in accordance with what is required by theory.

AMOUNT OF GAS CONSUMED BY THE THREE TYPES OF ENGINE HITHERTO IN PRACTICE.

1. Lenoir..95 cu. ft. per indicated HP. per hr.
   Hugon..85 " " " "
2. Brayton.50 " " " "
3. Otto....21 " " " "

For the Lenoir and Hugon engines the results of experiments by Mr. Tresca, of Paris, have been taken, as stated by Professor Thurston, corrected for an error into which he has fallen. He states the consumption of the engine to be 32 cubic feet per IHP. per hour, and then goes on to say that on the brake 4 HP. is obtained, while 8.6 is indicated. He has neglected to deduct from the gross indicated power in the cylinder, the pump resistance, and thus calculates the consumption on the gross indicated, instead of on the available indicated power. The available indicated power is not more than 5.2 HP., and the consumption is not less than 50 cubic feet per IHP. per hour.

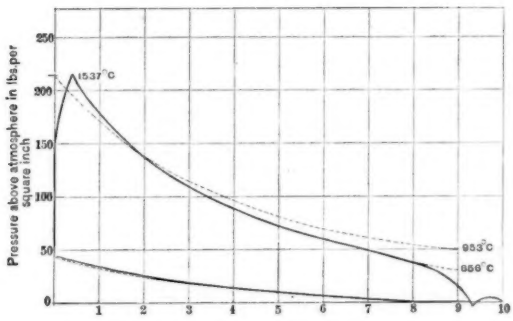
For the "Otto" engine have been taken the figures given by Mr. F. W. Crossley. It is seen that the results are much what would be anticipated from the theory already developed. The difference between types 1 and 3 is greater than theory would indicate; but at the time the Lenoir engine was in use, the imperfection of the igniting arrangements and the rapid heating of piston, and consequently of the entering gases, made its action diverge much more widely from theory than in the case with the "Otto." The latter engine not only has the advantage of a better theoretical cycle, but the arrangements are of a nature to secure a greater perfection of action, and consequently a still closer approach to theory. An amount of about 18 per cent. of the heat used by it is converted into work, but only 3.9 per cent. by the Hugon engine.

In types 1, 2 and 3, which have been discussed, it has been assumed that in each case the expansion doing work was carried to twice the volume of the air before compressing.

Fig. 6 is a diagram from one of the author's engines which belongs to type 3. It will be observed that in this engine the expansion is only continued until the volume of the hot gases becomes equal to the volume before compression.

Now the work actually given by 1 cubic foot of combustible mixture in the author's engine, as will be seen from Fig. 6, is 6,851 foot-lbs. The full lines are the diagram lines from the engine; the dotted lines are the lines of compression

Fig.6.



PARTS OF THE STROKE.

Diagram from Clerk's Gas Engine, cylinder, 6 ins. diameter, 12 ins. stroke, 15[illegible] revolutions per minute. Mean available pressure 70.1 lbs., 9 IHP. The maximum pressure is 220 lbs. per square inch above atmosphere. The pressure before ignition is 41 lbs. per square inch above atmosphere. The lower dotted line shows compression without loss of heat, to the same volume as exists in clearance space. Temperature before compresson 17°.3 C. (60° F.) Temperature after compression 150°.5 C. The upper dotted line shows the work done by air heated to 1,537° C., supposing it to lose no heat during expansion, except by doing work. The actual diagram shows a mean pressure during nine-tenth of stroke of 78 lbs. on the square inch, which is equal to 6,851 foot-lbs. per cubic foot of combustible mixture used. The dotted lines show an available pressure of 89.8 lbs. per square inch, which is equal to 7,888 foot-lbs. per cubic foot of air compressed. Duty $=\frac{7,888}{24,416}=0.323$.

Taking the amount of work to be obtained from a cubic foot of air compressed to 40 lbs. above the atmosphere, and then heated to 1,537° Centigrade, expanding as the piston moves to its volume before compression, and then exhausting, it will be found to give the following results:

1 cubic foot of air (17° Centigrade and 760 milimeters mercury) at constant volume requires to heat it from the temperature of compression 150°.5 Centigrade to 1,537° Centigrade, heat equivalent to	24,416 foot-lbs.
Maximum pressure in lbs. per square inch above atmosphere.....	220 lbs.
Pressure at end of stroke in lbs. per square inch.....	49 "
Mean pressure during available part of the stroke above atmosphere.....	89.8
Temperature at the end of the stroke.....	953° Centigrade.
Work done on the piston..	7,888 foot-lbs.

Duty $\frac{7,888}{24,788}=0.32$

and expansion without loss or gain of heat, except by work done on or by the air under similar conditions of temperature and compression. It will be observed that the compression line and the dotted line are very close together; no heat seems to be lost to the sides of the cylinder during compression; the loss of heat to the water-jacket is balanced by the gain of heat from the piston, which must necessarily be much hotter than the cylinder sides, as it only loses heat by contact with the cylinder and by the circulation of air in the trunk. The temperature attained at the commencement of the stroke is in both cases identical, 1,537° Centigrade; the temperature at the end of the stroke without loss of heat is 953°; the temperature in the cylinder at the end of the stroke is 656° Centigrade. The diameter of the cylinder from which this diagram was taken is 6 inches, and ahe length of stroke 12 inches. This flppears a very small loss of heat from a tame filling the cylinder, considering the surface exposed and the great difference

of temperature between the ignited gases and the enclosing walls. Is it to be concluded, then, that the loss of heat to the cylinder during the time of the forward stroke is only 953°−656°=297° Centigrade? On this assumption the duty of the engine would be—

$$\frac{6,851}{24,416}=0.286,$$

and the consumption of gas per indicated HP. per hour would be—

$$\frac{3.92}{0.286}=13.7 \text{ cubic feet,}$$

but the consumption is 22 cubic feet per indicated HP. per hour, so that there has in some way been lost much more heat than is to be accounted for by the temperatures as determined by the diagram. The duty of the engine is—

$$\frac{3.92}{22}=0.178.$$

The duty of the engine expanding to the same volume as the mixed gases before compression is—

	Gas required per IHP. per hr.	
		Cub. ft.
Duty without loss of heat to sides of cylinder	0.323	12.1
Duty with loss of heat as shown by diagram	0.286	13.7
Duty as determined by experiment	0.178	22.0

Now the number of cubic feet of combustible mixture required to produce 1 HP. for one hour in the author's engine is—

$$\frac{1,980,000}{6,851}=289.$$

The amount of gas in the engine per cubic foot of mixture, $\frac{22}{289}=0.0761$ cubic foot, or $\frac{1}{13}$ of the total volume of gaseous mixture passed into the engine. If only the amount of gas necessary to heat the air to the required temperature is present, 1 cubic foot requires 0.0482 cubic foot of coal gas, or about $\frac{1}{21}$ of its volume; that is, although to heat up a cubic foot of inflammable mixture from 150° Centigrade to 1,537° only 0.0482 cubic foot of coal gas is required, yet although there is present 0.0761 cubic foot, or 1.58 time the amount necessary, the temperature does not rise any higher. Why is this?

Before going into the question, it is better to determine as nearly as possible what becomes of 100 heat units used by the engine. The exhaust being discharged at a temperature of 656°, and the temperature of the air before compression being assumed at 17°, it follows that the exhaust from 1 cubic foot carries away with it (656−17) × 17.61 = 11,253 foot-lbs.

The work done by the cubic foot of mixture is 6,851 foot-lbs., and the equivalent in foot-lbs. of the gas present in 1 cubic foot of explosive mixture is 0.0761 ×505,000=38,430 foot-lbs. The heat is therefore disposed of as follows:

	Foot-lbs.	Heat-units percent.
Work done by 1 cubic foot of mixture	6,851	17.83
Mechanical equivalent of heat discharged with the exhaust	11,253	29.28
Mechanical equivalent of heat passing through sides of cylinder	20,326	52.89
	38,430	100.00

This investigation is only approximate. The determination, with anything like possible physical accuracy, would require an examination of many points involving months of continuous work. It is the author's intention to make an accurate research into the phenomenon attending the use of the gas engine, for the purpose of obtaining the physical constants necessary to calculate exactly the consumption of any power, size, and theory of gas engine, such as it may be possible to construct in the future. For the present, however, it is only necessary to discuss the principles in such a manner us to clearly show where original research is required. More than one-half of the total heat given to the engine passes through the sides of the cylinder and is lost. How is this enormous loss of heat sustained, while only a comparatively small fall of temperature takes place below the adiabatic curve?

This leads back to the question of the gas present in excess of the amount necessary to raise the temperature to 1,537° which has already been noticed. At this point it is necessary to consider the gas engine as something different from a hot-air engine.

The chemical phenomena attending combustion now require consideration. If 2 volumes of hydrogen be mixed with 1 volume of oxygen (the proportions necessary for complete combination of both gases to form water), and be ignited in a closed vessel in such a manner that the maximum pressure may be measured, it will be found that the pressure is a much lower one than would be expected if the complete combination of the two gases took place at once, and the whole heat due to this combination were developed. That this is not due to loss of heat to the sides of the vessel has been shown by Bunsen. He proved that the ratio of rise in pressure is exceedingly rapid compared to the rate of fall of pressure. The time taken for the inflammation of the whole volume of mixture is the time of attainment of the maximum pressure. In his experiments he used only a very small tube, which contained a volume of gaseous mixture, 8.15 centimeters long, by 1.7 centimeter in diameter, and the entire length of this column was traversed by the electric spark, in order that the inflammation of the whole mass in the tube might be as nearly instantaneous as possible. In practice he succeeded in producing a maximum temperature in so short a time as $\frac{1}{1000}$ part of a second. By examining the light from the explosion through a revolving disc provided with radiating segments, the rate of revolution of the disc being known, he determined the duration of light within the tube, and therefore the duration of a temperature not far removed from the maximum.

The duration of the illumination was found to be $\frac{1}{6}$ of a second. A maximum pressure, obtained in so short a time, with a duration so relatively long, makes it impossible that loss of heat through the sides of his tube could have affected his experiments. The cause, therefore, of the pressure falling so far short of what it would be if the combination took place completely, is simply this, that the temperature is so high that complete combustion is impossible. The temperature, and therefore the pressure produced by the combination of any gases, is limited by the dissociation or decomposition of their products of combustion.

When any two gases combine, say (H) and (O) to produce water, what happens is this. The temperature rises till a point is reached, when any further rise would decompose the water which is already formed; and if the gases are kept at this temperature, no further combination will take place. If the temperature is lowered, further combination takes place until it is low enough to allow of the existence of steam without decomposition.

The temperature at which steam can exist as steam without its partial resolution into hydrogen and oxygen gases is not a high one. At 960° to 1,000° Centigrade Deville has proved that it commences to decompose, and at 1,200° Centigrade, considerable decomposition takes place, the amount of decomposition increasing as the temperature rises: for each temperature there is a proportion of steam to free gases, which is constant, and does not change till the temperature changes. The same law holds true for carbon dioxide; at high temperatures it decomposes into carbonic oxide and free oxygen.

Bunsen attempted to determine the temperature attained on the explosion of a mixture of hydrogen and oxygen, a pure electrolytic mixture. He found that the maximum pressure attained by such a mixture is 10 atmospheres, the temperature before ignition being 5° Centigrade. From this he calculated the temperature produced, but in doing so, as Berthelot afterwards pointed out, he neglected the fact that when these gases combine, 3 volumes of the gases form 2 volumes of steam gas, and consequently if complete combination is assumed, and it be supposed that the pressure is produced by steam only, the volume, before ignition, must be calculated at two-thirds of that taken by the mixed gases. But as it is known that combination is incomplete, at the lowest assignable temperature of the combustion, and it is not possible to tell the amount of combination at a given pressure without knowing the temperature, this cannot be assumed.

As in determining temperature by an air thermometer it is necessary that the amount of air in the thermometer should be constant at the different temperatures, it is evident that the temperature of an explosion cannot be known from the increase in pressure unless the chemical changes taking place do not alter the volume of gases under observation.

In calculating the temperatures attained in the author's engine, this fact has been kept in view. The capacity of the space at the end of the cylinder was carefully taken by filling with water and weighing the water. As the proportion of the combining gases to the excess of oxygen or free nitrogen is very small, only one-thirteenth of the whole volume used being combustible gas, the space may be considered as simply filled with heated air, and the contraction caused by the formation of H_2O and CO_2 neglected, especially as an increase in volume follows the combination of the olefines with oxygen. 2 volumes of H combine with 1 volume of O, forming 2 volumes of steam. 2 volumes of marsh gas (CH_4) require for complete combustion 4 volumes of O, and form 4 volumes of H_2O and 2 volumes of CO_2. 2 volumes of carbonic oxide (CO) unite with 1 volume of O, forming 2 volumes of CO_2. If the olefines in coal gas be taken as of an average composition of C_2H_6, then 2 volumes require for complete combustion 9 volumes of oxygen, forming 6 volumes of H_2O and 6 volumes of CO_2.

Now taking the composition of coal gas as below the noted amounts of oxygen are required for combustion, and the given volumes of the products are formed—

vols.		vols.		vols.	
H=50	requires	25	O=50	H_2O	produced.
CH_4=33	"	66	O=99	CO_2&H_2O	"
CO=13	"	6.5	O=13	CO_2	"
C_2H_6= 4	"	18	O=24	CO_2&H_2O	"

100 + 115.5=225.5 gives 186 vols.

The amount of contraction due to complete combustion of this coal gas is small even when burning with pure oxygen, 225 volumes of the mixed gases becoming 186 volumes after combustion. When diluted with nitrogen the proportion of contraction is less and introduces no serious error. With a mixture of 1 volume of gas to 12 volumes of air, 125 volumes of the mixture before combination become 122 volumes when completely combined, at the original temperature, assuming the water to remain gaseous. If the curve of the dissociation of water and carbonic dioxide were known, it would be possible to show on the indicator diagram the reserve of heat available at each point of the fall.

What the engineer requires of the scientific chemist is a curve of the dissociation of water and carbonic acid, at temperatures ranging from the maximum produced by combustion down to the point at which it may be safely assumed that complete combination is possible.

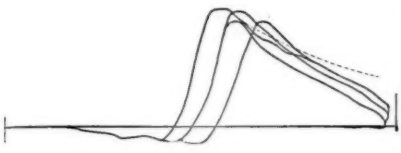
In Fig. 6 the dotted line shows a fall of temperature, by hot air doing work without loss of heat through the cylinder, and the black line shows the actual fall of temperature in the author's engine, with loss of heat through the sides of the cylinder. It is evident then that the cause of so near an apparent approach to theory is, that at the maximum temperature, complete combination of gases with oxygen is impossible, and cannot take place until the temperature falls. As the temperature falls the gases further combine, until a temperature is reached at which combination is complete.

The loss of heat through the sides of the cylinder is therefore much greater than would appear from the diagram. In calculating the efficiency of the gas engine, all previous observers have assumed that the loss of heat to the cylinder is to be obtained from the comparison on the indicator diagram of the actual expansion-line with an adiabatic line from the same maximum temperature and pressure. So far as the author is aware, Professor Rücker, of Leeds, was the first to point out the necessity of taking into account the phenomena of dissociation in making such comparisons. Accordingly, all previous estimates of efficiency, based on the indicator diagram, are much too high.

The gas engine, then, differs from the hot-air engine, using air heated in the manner assumed in the first part of this paper, in this, that the temperature is sustained, notwithstanding the enormous flow of heat through the sides of the cylinder, by the continuous combination of the dissociated gases.

Figs. 7 and 8, have been taken from the "Journal of the Franklin Institute." They are Lenoir engine diagrams, and in them the same phenomena are apparent; although running at a very slow speed, the pressure is most perfectly sustained, the dotted lines showing the adiabatic, and the full lines the actual diagram. The author of the paper in which they occur, gives the probable maximum tem-

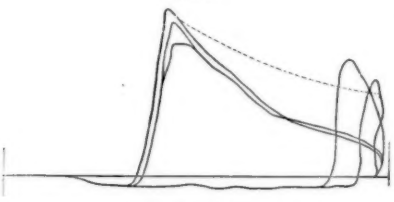
Fig.7.



LENOIR ENGINE.

Diagram at 50 revolutions, cylinder 8⅛ inches diameter, 16¼ inches stroke.

Fig.8.



LENOIR ENGINE.

Diagram at 45 revolutions, 1 inch=32 lbs.

perature attained at about 1,356° Centigrade, and he says, "The dotted line represents the theoretical curve of expansion, taking into account the loss of heat and consequent fall of pressure, due to the work done (which is the proper theoretical curve for an indicated diagram). The temperature at the end of the stroke, indicated by this line, would be 2,156° Fahrenheit (1,180° Centigrade). The final temperature shown by the diagram, supposing there be no leakage, is 1,438° Fahrenheit (781° Centigrade), and the difference 718° Fahrenheit (399° Centigrade), is the quantity of heat absorbed by the water-jacket by which the cylinder is surrounded."

"It will be observed that the explosion takes place so late in the stroke that there is a considerable available pressure at the end of the stroke, which of course is not utilized."

Now if the Lenoir engine had only lost this amount of heat through the sides of the cylinder it would have been very economical, and would have approached the theoretic consumption mentioned in the earlier part of this paper; but the causes of loss are so great that it never did come anything near this figure, and an error is introduced through neglecting the effects of dissociation.

Interesting information, however, is to be obtained from these diagrams as to the proportion of gas and air in the mixture used by the Lenoir engine. When these diagrams were taken the maximum temperature after ignition was 1,356° Centigrade; now in the author's present engine the maximum temperature is 1,537°; it follows that Lenoir used a more diluted mixture as the temperature after ignition was lower. The engine giving this diagram could not have been using an ignitable mixture containing more gas than one-fourteenth of its volume—a mixture which the author finds to be easily ignited at ordinary atmospheric pressure. The statement is ofte made that such a mixture will not explode except it be first compressed; this is incorrect, it is possible to ignite even a weaker mixture without compression. Coquillon has determined the limits between which a mixture of marsh gas (CH_4) and air can be exploded. Mixtures of marsh gas and air in different proportions were introduced into a eudiometer and fired by the electric spark, with the following results:

Marsh gas 1 volume, air 5 volumes. The spark is without effect. Marsh gas 1 volume, air 6 volumes. Explosion only occurs in a succession of shocks. This is the first limit of possible explosion; the marsh gas is in excess. Marsh gas 1 volume and 7, 8 and 9 volumes of air give a sharp explosion. With 12, 13, 14, 15 volumes of air for 1 volume of marsh gas the explosion occurs, but grows gradually weaker. With 16 volumes of air the effect is reduced to a series of slight intermittent commotions. This is the second limit; the air is in excess.

In Fig. 8, ignitions will be observed very late in the stroke; these misses were caused by the points between which

the electric spark is discharged getting wet and thus preventing the passage of the spark at the proper time. From these diagrams, the time, from the beginning of rise in pressure to the attainment of maximum pressure, is found to be from one twenty-seventh to one-thirtieth of a second; when the ignitions are late it takes longer, one-twentieth of a second being required; that is, the flame has spread completely through the mass in one-twentieth part of a second.

Now in the author's engine, calculating from the moment when the ignition port is opening to the flame, to the moment of maximum pressure as found from the diagrams, it has been ascertained that the time occupied is an average of one twenty-fifth of a second, a time nearly identical with that found for the Lenoir engine.

If it be admitted that the flame has spread completely through the mass when the maximum pressure is attained in the Lenoir engine, it cannot be supposed that it has not spread in like manner throughout the mass of ignitable mixture in the modern compression engine. Maximum pressure is the only outward indication of complete inflammation; by complete inflammation is not meant the thorough chemical combination of the active gases present, but the spread of the flame through the entire mass. That when maximum pressure has been reached complete inflammation has also been attained has hitherto been considered self-evident. It is only lately that the theory has been advanced by Mr. Otto that in the modern compression engine attaining maximum pressure at the beginning of the stroke, the flame has not spread throughout the mass of the ignitable mixture in the cylinder; but that as the piston moves forward the pressure is sustained by the gradual spread of the flame. This supposed phenomenon has been erroneously called slow combustion; if it has any existence it should be called slow inflammation. It has a real existence in the Otto engine only when it is working badly; but even then maximum temperature is attained, and very distinctly marks the point of completed inflammation.

The time taken to attain maximum pressure is longer in a large engine than in a small one, because the distance through which the flame has to travel is greater. During the investigation already referred to, Professor Bunsen determined the celerity of the propagation of ignition through a pure explosive mixture of hydrogen and oxygen in the following manner: the explosive mixture was allowed to burn from a fine orifice of known diameter, and the current of the rate of the gaseous mixture was carefully regulated by diminishing the pressure, to the point at which the flame passed back through the orifice and ignited the gases below it. This passing back of the flame occurs when the velocity with which the gaseous mixture issues from the orifice is inappreciably less than the velocity with which the inflammation of the upper layers of burning gas is propagated to the lower and unignited layers.

The rate of the propagation of the ignition in pure hydrogen was found to be 34 meters per second. In a maximum explosive mixture of carbonic oxide and oxygen it was not quite 1 meter per second.

Mr. Mallard has determined the rapidity of the propagation of inflammation through mixtures of coal gas and air by this method, and found that the maximum rate of propagation was attained with a mixture of 1 volume of coal gas with 5 volumes of air, and it is 1.01 meter per second. One volume of coal gas with $6\frac{1}{2}$ volumes of air gave a rate of 0.285 meter, or 11 inches per second.

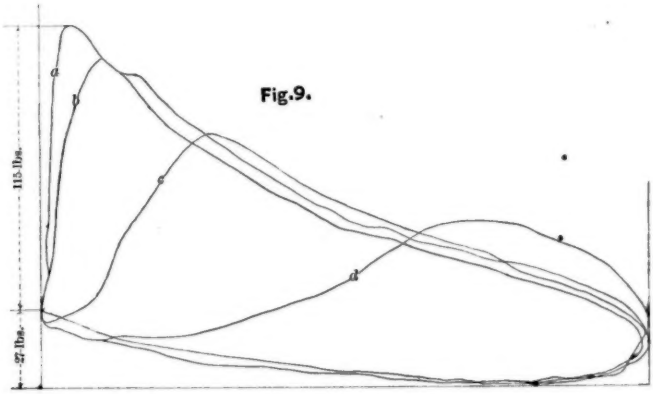
This is the rate of ignition, it must be remembered, at constant pressure; in a closed tube fired at one end it would ignite with much greater rapidity. In a closed space the conditions of inflammation are quite different. The ignited portion instantly expands, compressing the portion still remaining, and thus carries the flame further into the mass, so that to the rate of ignition at constant pressure is added the projection of the flame into the mass by its expansion. To determine from the rate of ignition at constant pressure the time necessary to completely inflame a given volume of mixture at constant volume is a very complicated problem, which it is probable can only be solved experimentally.

The author has found it possible to ignite a whole mass in any given time between the limits of one-tenth and one-hundredth part of a second, by so arrang-

ing the plan of ignition that a small volume of gaseous mixture is first ignited, expanding and projecting a flame through a passage into the mass of inflammable mixture, and thus adding to the rate of ignition the mechanical disturbance produced by the entering flame. He has succeeded by this means in producing maximum pressure in one-hundredth part of a second in a space containing 200 cubic inches. This rate of ignition is too rapid, and would not give the engine time to take up the slack in bearings, connecting rods, &c. But by firing a mixture with varying amounts of mechanical disturbance almost any time of ignition can be obtained between $\frac{1}{100}$ and $\frac{1}{10}$ of a second. It does not matter whether the mixture used is rich or weak in gas; the rich mixture can be fired slowly and the weak one rapidly, just as may be required. The rate of ignition of the strongest possible mixture is so slow that the time of attaining complete inflammation depends on the amount of mechanical disturbance permitted.

Fig. 9, a diagram from an Otto engine, shows what happens in a compression engine of type 3 when the ignition comes late and the movement of the piston overruns the rate of the spread of the flame. It is then seen that the maximum pressure is not attained until far on in the stroke, and as a consequence great loss of power results, the pressure attaining its maximum when it is time for the exhaust valve to open. This may happen from several causes, a too diluted mixture, or too little mechanical disturbance by the entering flame; or the ignition may be missed until the pressure begins to fall by the forward movement on the piston, when the rate of inflammation begins to come more nearly to Mallard's number of 11 inches per second. This slow combustion, or rather slow inflammation, is to be avoided in the gas engine. Every effort should be made to secure complete inflammation as soon after ignition as is practicable. The lines in the diagram show this very clearly;

Fig.9.

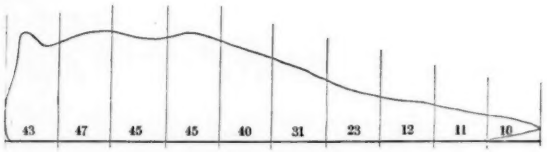


COMPRESSION GAS ENGINE.

the normal lines are those in which the rise is almost straight up from the point of the beginning of the ignition; they are marked *a* and *b*; the line *c*, although commencing from the beginning of the stroke, does not record the maximum pressure till the piston has moved forward one-third of its stroke, while the line *d* does not depart from the compression line until one-tenth of the forward movement, and does not attain its maximum till near the end of the stroke. In the last case the ignition has been missed until the piston is in rapid motion, and consequently the flame is at first unable to overtake it. The rate of inflammation at constant pressure has been determined only for atmospheric pressure; were it known for higher pressures it would be possible to calculate exactly the piston speed which would prevent any rise in pressure at all.

Fig. 10 was taken by the author from the motor cylinder of an American Brayton engine of type 2. It shows how the pressure is sustained as the ignited gases enter the motor cylinder in flame. This is the true slow inflammation engine; in it the pressure after ignition is not allowed to rise, but only increase of volume takes place; at about the middle of the stroke the supply of flame is cut off and the piston moves on, and the heated gases expand doing work.

Fig. 10.

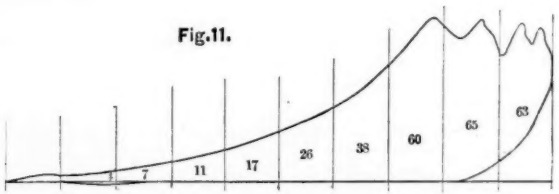


BRAYTON PETROLEUM ENGINE (MOTOR CYLINDER).

Area of piston, 50.26 inches. Stroke, 12 inches. Mean pressure, 30.2 lbs.

Fig. 11 is the compression pump diagram, which must be deducted before getting the available indicated power. The motor-piston was of the same area as the pump, but had double the length of stroke. This type of engine is not a good one for a cold cylinder, the loss of heat through the cylinder being much more than in type 3; but, as it has been before said, the possibility of using the theory in the future with a hot piston and cylinder renders reference to this engine interesting. Slow inflammation is a mistake if applied to engines of types 1 and 3 with cold cylinders; in type 1, if the piston were moving rapidly enough, the inflammation could be so slow that with a perfectly sustained temperature no power at all could be obtained. That is, the air would simply expand in volume without rising in pressure above the atmosphere, and even without loss of heat to the sides of the cylinder the whole heat would be uselessly discharged.

Fig. 11.



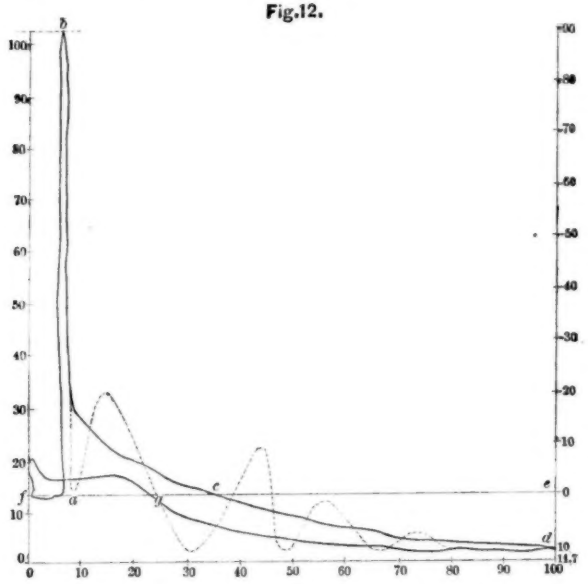
BRAYTON PETROLEUM ENGINE.

Air-pump diagram. Area of piston, 50.26 inches. Stroke, 6 inches. Mean pressure, 27.6 lbs. Pressure in reservoir, 60 lbs.

In type 3 the perfection of slow combustion would be attained when the flame spread just as rapidly as the piston moves forward, and the pressure was never raised above that due to compression. The pressure diagram would then give the ideal results of "gradual expansion of gases" and a "perfectly sustained pressure." But this is just the condition of greatest loss of heat; sustained pressure means sustained, indeed increasing temperature, and the object to be attained in a good gas engine is to produce the most rapid possible fall of temperature due to work performed, to keep the mean temperature as low as possible, and it is only so far as this is successfully done that economy is possible. Slow inflammation causes loss of heat and power; rapid inflammation reduces the loss to a minimum while attaining the maximum possible power.

One more engine may be noticed; its diagram is given at Fig. 12. In action it comes under type 1, but uses a very large amount of expansion, and is further complicated by cooling. It is the well-known Otto and Langen engine of the free piston type; in it gas and air are taken in for a portion of the stroke at atmospheric pressure and then ignited while the piston remains at rest until the pressure sets it in motion; the piston is free to move apart from the shaft altogether, and on the up-stroke it does no work.

Fig. 12.



OTTO AND LANGEN ENGINE (FREE PISTON).

Percentage of stroke.

From *f* to *a* air and gas are taken into the cylinder. At *a* the mixture is ignited and the piston moves to *c* with considerable velocity when the pressure has fallen to the atmosphere. From *c* to *e* it continues to move with continually diminishing velocity, until at *e* it comes to rest and then returns doing work, the work being equal to the diagram *d g e* added to the weight of the piston and rack through the stroke. It will at once be seen that as the gases only do work on the piston from *a* to *c*, and this work is absorbed in giving a certain velocity to the piston, and from *c* to *e* the velocity of the piston is being gradually checked by doing work on air, assuming the piston to have no weight, the area of the portion of the diagram *a c b* must be equal to the part *c e d*.

It is evident that the lines in the diagram are incorrect; the explosion cannot fall nearly so rapidly as shown; *c* should be much nearer *e*. The oscillations of the indicator have been so great that accuracy is impossible. The fall of the line *d g* below *d e* is caused by the cooling of the gases on the return stroke.

In this engine the advantage consists more in the large amount of expansion than the velocity of the forward movement of the piston.

The diagram has been taken from a paper by Mr. F. W. Crossley; with reference to it he says:

"The very sudden and extreme rise in pressure at the moment of explosion is due simply to the expansion of the gases under the temperature of the flame. If this temperature be taken at 5,000° Fahrenheit, and divided by 520 for the rate of expansion from an initial temperature of about 60°, it gives an expansion

of about 10 times; and as the gas compound occupied one-eleventh of the cylinder at the moment of ignition, if it expands ten times it gives very nearly the stroke actually taken by the piston. The 5,000° is an assumption only, but seems to be confirmed by the amount of expansion which follows it. After the explosion the temperature falls almost instantaneously, as shown by the sudden drop of pressure in the diagram."

In the author's opinion Mr. Crossley has completely misinterpreted his diagram. Taking the temperature before ignition at 60° Fahrenheit, and the maximum pressure shown on the diagram as 100 lbs. absolute, it follows that the maximum temperature is not greater than 2,900° Fahrenheit (1,590° Centigrade). It is difficult to see how 5,000° Fahrenheit can be assumed. The expansion of the gases by the extreme movement of the piston following ignition has no necessary relation to the temperature of the explosion; but it is determined wholly by the work done on the piston by the explosion between the maximum and atmospheric pressures. Whenever the gases in the cylinder fall to the pressure of the atmosphere, which happens according to the diagram at about 0.35 of the stroke, the piston is doing work on air, and the mean pressure below the atmosphere from *c* to *e* is the exact measure of the work previously done on the piston by the explosion, which has been expended in giving the piston velocity. This energy of motion is now being expended by compressing the atmosphere. Taking into consideration the weight of the piston and friction of the rings, rack and clutch, it is certain that the area of the part of the diagram *a b c* must be considerably greater than *c e d*; in the diagram it appears much less. It should be greater by the amount of work expended in giving the piston energy of position, and the amount lost by friction on the up-stroke.

As a means of showing the nature of the explosion this diagram is misleading; it is certain that the maximum pressure was less, and that the fall of pressure is nothing like so rapid as it there appears. Comparing Fig. 12 with Figs. 7 and 8 the difference in appearance is so striking that it looks as if in one case the fall in pressure was instantaneous and in the other gradual; this would be remarkable, considering that the maximum temperatures are very similar. If the lines in Fig. 12 be corrected and drawn with the same relation of scale between pressures and strokes, it will be found to be very similar in appearance to Figs. 7 and 8, so far as rate of fall is concerned. Indeed the advantage claimed for this engine is a movement of piston so rapid that its expansion is complete before much heat is lost to the sides of the cylinder, which is inconsistent with a fall of pressure more rapid than in the Lenoir engine.

To go completely into the points of originality in these engines would require a paper on the "History of the Gas Engine;" but it may be well to state the name of the first to propose each type:

		Year.
Type 1.	Explosion acting on piston connected to crank...W. L. Wright	1833
	Explosion acting on free piston, Barsanti & Matteuci	1857
Type 2.	Compression after ignition but at constant pressure. C. W. Siemens	1860
	Compression with increase in volume.....F. Millon	1861
Type 3.	Compression with increase in pressure.....F. Millon	1861
	After ignition but at constant volume.....	

So far as the author has been able to ascertain, these are the names of the first to propose distinctly each of the three types of gas engine.

From the considerations advanced in the course of this paper, it will be seen that the cause of the comparative efficiency of the modern type of gas engines over the old Lenoir and Hugon is to be summed up in one word, "compression." Without compression before ignition an engine cannot be produced giving power economically and with small bulk. The mixture used may be diluted, air may be introduced in front of gas and air, or an elaborate system of stratification may be adopted, but without compression no good effect will be produced.

The proportion of gas to air is the same in the modern gas engine as was formerly used in the Lenoir, the time taken to ignite the mixture is the same, the only difference is compression. The combustion, or rather the rate of inflammation, is indeed quicker in the modern engine because the volume of mixture

used at each stroke is greater, and yet the time taken to completely inflame the mixture is no more than in the old type. The cause of the sustained pressure shown by the diagrams is not slow inflammation (or slow combustion as it has been called), but the dissociation of the products of combustion, and their gradual combination as the temperature falls, and combination becomes possible. This takes place in any gas engine, whether using a dilute mixture or not, whether using pressure before ignition or not, and indeed it takes place to a greater exsent in a strong explosive mixture than in a weak one.

The modern gas engine does not use slow inflammation (or slow combustion if the term be preferred), but when working as it is intended to do, completely inflames its gaseous mixture under compression at the beginning of the stroke. By complete inflammation is meant complete spread of the flame throughout the mass, not complete burning or combustion. If by some fault in the engine or igniting arrangement the inflammation is a gradual one, then the maximum pressure is attained at the wrong end of the cylinder, and great loss of power results.

Compression is the great advance on the old system; the greater the compression before ignition the more rapid will be the transformation of heat into work by a given movement of the piston after ignition, and consequently the less will be the proportional loss of heat through the sides of the cylinder. The amount of compression is of course limited by the practical consideration of strength of the engine and leakage of the piston, but it is certain that compression will be carried advantageously to a much greater extent than at present. The greatest loss in the gas engine is that of heat through the sides of the cylinder, and this is not astonishing when the high temperature of the flame in the cylinder is considered. In larger engines using greater compression and greater expansion it will be much reduced. As an engine increases in size the volume of gaseous mixture used increases as the cube, while the surface exposed only increases as the square, so that the proportion of volume of gaseous mixture used to surface cooling is less the larger the engine becomes. Taking this into consideration, it may be accepted as probable that an engine of about 50 indicated HP. could be made to work on 12 cubic feet of coal gas per indicated HP. per hour, or a duty of about 32 per cent.

The gas engine is as yet in its infancy, and many long years of work are necessary before it can rank with the steam engine in capacity for all manner of uses; but it can and will be made as manageable as the steam engine in by no means a remote future. The time will come when factories, railways amd ships will be driven by gas engines as efficient as any steam engine, and much more safe and economical of fuel. Grs generators will replace steam boilers, and power will not be stored up in enormous reservoirs, but generated from coal direct as required by the engine.

The steam engine converts so small an amount of the heat used by it into work that, although it was the glory and honor of the first half of the century, it should be a standing reproach to engineers and scientists of the present time having constantly before them the researches of Mayer and Joule.

APPENDIX.

DATA USED IN THE PAPER ON "THE THEORY OF THE GAS ENGINE."

Specific heat of air at constant volume.	= 0.169 ; water 1.00
Specific heat of air at constant pressure	= 0.238
Mechanical eqivalent of heat foot-lbs. Centigrade.....	=1389.6
Specific heat of air at constant volume in foot-lbs. for 1 cubic foot at 17° C. and 760 mm. barometer.....	17.6 foot-lbs.
Specific heat of air at constant pressure in foot-lbs. for 1 cubic foot from 17° C. and 760 mm.....	24.8 "
Weight of 1 cubic ft. of air at 17° C. and 760 mm.....	0.075 lb.

Burning completely in oxygen, the following substances are taken as evolving the noted amounts of heat in Centigrade units, per unit weight of substance burned.

Hydrogen	34,170
Carbon	8,000
Carbonic oxide	2,400
Marsh gas	13,080
Olefiant gas	11,900

REPORT ON THE INCANDESCENT LAMPS EXHIBITED AT THE INTERNATIONAL EXPOSITION OF ELECTRICITY, PARIS, 1881.*

From "The Engineer."

I.—Description of the Lamps.

The only lamps in the Exhibition which were purely incandescent in character were those of Edison and Maxim, in the United States section, and those of Swan and Lane-Fox, in that of Great Britain. The idea represented in these lamps is essentially the same in all of them, the differences being, for the most part, details of construction. They all consist of a glass envelope more or less spherical in form, in which is enclosed a carbon loop made of carbonized organic material, and supported upon wires of platinum sealed into the glass. The space in the interior of the lamp is very perfectly exhausted.

A. *The Edison Lamp.*—The Edison lamp is pear-shaped in form. The carbon filament is long and fine, and is bent into the shape of a U. It is made from Japanese bamboo, cut to the requisite size in a gauge. In section it is nearly square, being about 0.3 milimeter on a a side, the ends being left considerably wider. The fiber is carbonized in moulds of nickel, and is attached to the conducting wires by copper, electrolytically deposited upon them.

B. *The Swan Lamp.*—The Swan lamp is globular in form, the neck being quite long. The carbon filament is made from cotton thread, parchmentized before carbonization by treatment with strong sulphuric acid. The ends of this filament are very much thickened, and the loop has a double turn at the top. Its ends are clamped in a pair of metal holders, supported laterally by a stem of glass which rises through the neck to the base of the globe. Below, these holders are fastened to wires of platinum which pass through the glass.

C. *The Maxim Lamp.*—The Maxim lamp is also globular in form, but it has a short neck. Within the neck rises a hollow cylinder of glass, supporting upon its summit a column of blue enamel, through which pass the conducting wires of platinum which carry the carbon. The filament is made from cardboard cut by a punch into the form of an M. In section, therefore, it is rectangular, and several times as broad as it is thick. It is carbonized in a mould through which a current of coal gas is passed. After carbonization the filament is placed in an attenuated atmosphere of hydrocarbon vapor and heated by the current. The vapor is decomposed, and its carbon is precipitated upon the filament. In this way not only are inequalities obliterated, but the resistance of the filaments may be equalized, and brought to any standard required.

D. *The Lane-Fox Lamp.*—The Lane-Fox lamp is ovoid in shape, the neck being in length intermediate between the two lamps last described. The carbon is in the form of a horseshoe, and is circular in cross section. It is made from the root of an Italian grass, largely used in France for making brooms. After carbonization the filaments are classified according to their resistances. They are then heated in an atmosphere of coal gas, by which carbon is deposited upon them, as in the filaments of the lamps last described. The filament in the lamp is supported by platinum wires, to which it is attached by sleeves of carbon encircling both. These wires pass through tubes in the top of a hollow glass stem. Just below the extremities of these tubes are two small bulbs containing mercury, forming the contact between the platinum wires sealed into the glass above and the copper conductor which enters from below. These conductors are held in place by plaster, which fills the base of the lamp.

II.—Methods of Measurement.

The question to be determined was simply the efficiency of these lamps. The efficiency of a lamp is the ratio of energy produced to energy consumed, *i. e.*, the quantity of light given by the lamp for each horse-power of current which it

* By an Experimental Committee, consisting of Messrs. George F. Barker, William Crookes, and others.

consumes. The data required to calculate this efficiency may be obtained when the electromotive force of the current, the resistance of the lamp when giving its light, and its illuminating power have been determined.

1. *Electromotive Force.*—The electromotive force, or fall of potential through the lamp, was measured by Law's method. A suitable condenser was charged by being put in communication with a standard Daniell cell, and then discharged through a high resistance galvanometer, the deflection of the needle being noted. This condenser was then connected to the two wires of the lamp, and again discharged through the galvanometer, the deflection being made the same as before by means of a variable shunt connected with the galvanometer. Since, with a given condenser, the charges it receives are proportional to the potentials of the charging currents, and since the discharge deflections of a galvanometer represent the quantity of these charges, it follows the electromotive forces are proportional to these discharge deflections. If, however, as in the present case, the discharge deflections are made equal by means of shunts, then the electromotive forces are proportional to the multiplying power of the shunts.

2. *Resistance.*—The resistance of the lamp, when giving its light, was obtained by making the lamp one side of a Wheatstone's bridge through which the main current was flowing. The second and fourth sides were formed of fixed resistances of known value, and the third side of an adjustable resistance. When the bridge is balanced the product of the two fixed resistances, divided by the adjusted resistance, gives the resistance of the lamp at the given candle power.

3. *Illuminating Power.*—The illuminating power of the lamp was measured on a Bunsen photometer. At one end of the bar was the lamp itself, at the other two standard candles, placed nearly in line. The plane of the carbon filament was placed at 45 deg. to the length of the bar, and each lamp was measured at 16 and 32 candles.

III.—Apparatus Employed.

1. *Condenser.*—The condenser used in these measurements had a capacity of 1 microfarad, divided into sections of 0.4, 0.3, 0.2, and 0.1. The dielectric was paraffined mica, and the brasswork was supported on ebonite pillars. Made by Latimer Clark, Muirhead, and Co., London, and exhibited in their section at the Exhibition.

2. *Galvanometer.*—The galvanometer was a Thomson double-coil astatic instrument, enclosed in a square case with glass sides. Measured resistance, 6550 ohms. Used with lampstand and scale in the ordinary way. Made by Elliot Brothers, London.

3. *Standard Cell.*—An ordinary Daniell cell, the copper plate being immersed in a saturated solution of pure copper sulphate, contained in the porous cell, and the zinc plate amalgated in a saturated solution of pure zinc sulphate in the outer jar. One of a battery of ten cells forming a part of the Edison exhibit.

4. *Resistance Coils.*—(*a*) A set of standard coils, measuring from 1 ohm to 5000 ohms. All other resistances employed were standardized by these. Made by L. Clark, Muirhead, & Co., and a part of their exhibit. (*b*) A set of coils used in the Wheatstone's bridge. Compared carefully with set (*a*). These coils formed a part of the exhibit of Edison.

5. *Wheatstone's Bridge.*—Four conducting wires of large size arranged on the table in the form of a rhomb. A test golvanometer was inserted between the obtuse angles of the rhomb, and a pair of shunt wires from the main conductors were attached at the acute angles. The first side of the rhomb contained the lamp to be measured, standing in its place on the photometer; the second side contained a fixed resistance of 5 ohms; the third side contained a variable resistance (resistance *b*); and the fourth side a fixed resistance of 950 ohms. This bridge formed a part of the Edison exhibit.

6. *Photometers.*—The photometer employed was of the Bunsen form, having a double bar, 80 in. long, graduated in inches and in candles. The disc was of parraffined paper, with a plain spot in the center. The disc box was movable on rollers, and contained inclined mirrors to facilitate the adjustment. The candles used were of spermaceti, made by Sugg, of London, to burn 120 grains—7.776 grms.—per hour. The entire apparatus was surrounded with heavy black cloth. Also a part of the Edison exhibit.

7. *Dynamo-Electric Machine.* — An Edison 60-light machine was used to furnish the current required. In this machine the field magnets, which are very long and heavy, stand vertically. The field is maintained by a shunt current, regulated by an adjustable resistance in its circuit. The bobbin is wound on a cylinder like that of Siemens, from which it differs, however, in its details. Its resistance was only 0.03 ohm, and the current delivered, at a speed of 900 revolutions, had an electromotive force of 110 volts. A part of the Edison exhibit.

IV.—Resistance of Lamps Cold.

The resistance of the lamps cold was measured on a Wheatstone's bridge of the ordinary form and in the usual way. The Edison lamps were taken at random from the stock on hand. The Swan lamps were furnished by Mr. Edmunds, the Lane-Fox lamps by Mr. Stewart, and the Maxim lamps by Mr. Lockwood. Twenty-four of each were taken—except the Lane-Fox, of which only fifteen were furnished—and ten selected from these for the tests. The measurements of the Edison and Swan lamps were made by Mr. E. G. Acheson; those of the Lane-Fox and Maxim lamps by Mr. H. Crookes. The following are the results obtained:—

Number.	Edison.	Swan.	Lane-Fox.	Maxim.
1	237	74	53	73
2	233	50	56	84
3	268	54	56	76
4	260	73	56	74
5	251	55	54	74
6	228	72	50	71
7	227	39	53	68
8	249	67	52	63
9	219	55	57	65
10	237	52	63	73
Means,	241	59	55	72

V.—Measurement of Efficiency.

1. *Experimental Results.*

A. *The Edison Lamp.*—In this measurement the entire condenser was employed. When charged with the standard cell and discharged through the galvanometer without shunt, a deflection of 310 scale divisions was obtained, as a mean of ten closely accordant experiments. The photometer readings were made by Mr. Crookes, the bridge readings by Major R. Y. Armstrong, and the galvanometer readings by Prof. G. F. Barker.

(*a*) *At* 16 *candles.*

Number of lamp.	Photometer reading.	Bridge reading.	Galvanometer reading.
1	16—14.75	35—34.5	75
2	16—15	35.0	74
3	16	30.5	74
4	16	32.3	73
5	16—17	33.4	73
6	16 - 17.5	36.0	73
7	16—15	36.6	78
8	16	34.5	75
9	16—19	37.5	74
10	16	37.7	74

(*b*) *At* 32 *candles.*

1	32	37.2	66
2	32	37.2	65
3	32	33 2	66
4	32	34.3	64
5	32	35.2	67
6	32	37.9	69
7	32	38.5	69
8	32	36 3	69
9	32	38.9	69
10	32	38 8	69

B. *The Swan Lamp.*—The entire condenser was used in these measurements also, the deflection being 310 divisions. The photometer was read by Mr. H. Crookes, the bridge by Mr. Crookes, and the galvanometer by Professor Barker.

(*a*) *At* 16 *candles.*

Number.	Photometer.	Bridge.	Galvanometer.
1	16	119.5	136
2	16	161.7	145
3	16	148.8	137
4	16	113.5	122
5	16	145.9	134
6	16	122.1	138
7	16	229.0	179
8	16	135.1	145
9	16	159.5	146
10	16	171.0	145

(*b*) *At* 32 *candles.*

1	32	123.5	121
2	32	167.2	122
3	32	155.2	121
4	32	116.0	116
5	32	154.7	115
6	32	129.7	120
7	32	237.0	146
8	32	137 5	128
9	32	163 0	127
10	32	175.2	120

C. *The Lane-Fox Lamp.*—The entire condenser was employed, and the deflection was the same, 310 divisions. Mr. H. Crookes read the photometer, Mr. Crookes the bridge, and Prof. Barker the galvanometer.

(*a*) *At* 16 *candles.*

Number.	Photometer.	Bridge.	Galvanometer.
1	16	172.0	150
2	16	168.7	145
3	16	177.6	161
4	16	171.7	157
5	16	171.0	156
6	16	189 5	156
7	16	179.0	156
8	16	181.1	164
9	16	161 7	146
10	16	164 7	148

(*b*) *At* 32 *candles.*

1	32	178 7	135
2	32	175.5	129
3	32	181 2	149
4	32	175.2	148
5	32	175.7	143
6	32	192.3	143
7	32	186.2	146
8	32	184.5	146
9	32	167.3	133
10	32	172.0	129

D. *The Maxim Lamp.*—The entire condenser was used, as in the previous cases; but the deflection obtained was 315 divisions, owing probably to the higher temperature of the room. Photometer read by Mr. H. Crookes, bridge by Mr. Crookes, galvanometer by Prof. G. F. Barker.

(*a*) *At* 16 *candles.*

Number.	Photometer.	Bridge.	Galvanometer.
1	16	111.8	115
2	16	111.3	119
3	16	106.2	111
4	16	124 7	120
5	16	111.9	122
6	16	138.5	121
7	16	122.0	122
8	16	115 6	118
9	16	120.6	123
10	16	103.0	111

(*b*) *At* 32 *candles.*

1	32	114.6	102
2	32	114.8	106
3	32	109.7	100
4	32	128.6	112
5	32	114.5	113
6	32	140.8	113
7	32	126.9	110
8	32	120.4	105
9	32	126.5	110
10	32	109.7	105

E. *The Candle Record.*

	Candle-power.	Loss in Gram.	Time in Min.	Loss per Min.
1. Edison Lamp	16	18.13.	73	0.2483
	32	21.22.	84	0.2526
2. Swan Lamp	16 & 32	34.15.	126	0.2695
3. Lane-Fox Lamp	16 & 32	40.70.	153 75.	0.2647
4. Maxim Lamp	16 & 32	26.90.	104	0.2586

2. *Methods of Calculation.*

1. *Illuminating Power.*—The standard candle should burn 7.776 grms. spermaceti per hour, or 0.1296 grm. per minute. The two candles used should burn 0.2593 grm. per minute. The corrected candle power of the lamp, therefore, is obtained by the proportion: As 0.2502 is to the amount actually burned per minute, so is the observed candle-power to the corrected candle-power.

2. *Resistance* (*hot*).—From the theory of the Wheatstone bridge, the resistance of either side is equal to the product of the adjacent sides divided by the opposite side. In the bridge used for the measurement the resistances in the two adjacent sides were 950 and 5 ohms. Hence by dividing their product, 4750, by the reading of the variable resistance observed, the resistance of the lamp hot is obtained.

3. *Electromotive Force.*—In Law's method the electromotive forces are proportional to the multiplying power of the shunts employed Since with the Daniell cell no shunt was used, the multiplying power of the shunt used with the lamp-current represented directly the electromotive force through the lamp, in terms of the standard shell. The multiplying power of a shunt is the sum of the galvanometer resistance and the shunt resistance, divided by the shunt resistance. In this case the resistance of the galvanometer was 6550 ohms. Hence if S represents the resistance of the shunt, obtained by experiment, $\frac{6550 + S}{S}$ will represent the electromotive force. Since the electromotive force of a Daniell cell is not 1 volt, as here assumed, but 1.079 volts, strict accuracy would require the figures given to be increased in that ratio. Moreover, the small error arising from the inductive action of the needle on the galvanometer coils has been regarded as unimportant.

4. *Current.*—By the law of Ohm the current strength is the quotient of electromotive force by resistance. Dividing the electromotive force in volts by the resistance in ohms the current strength is obtained in Ampères.

5. *Electrical Energy*—The work done by a current is proportional to the product of the square of the current-strength into the resistance of the circuit. Or, since the electromotive force is equal to the product of the current-strength by the resistance, the energy is represented by the product of the electromotive force in volts by the current-stength in Ampères. This gives the energy in Volt-Ampères.

6. *Mechanical Energy.*—Since an absolute unit of work is done per second by an absolute unit of electromotive force in a circuit of one absolute unit of resistance, 1 Volt-Ampère represents 10^7 absolute units of mechanical work per second, or 0.10192 kilogrm.-meter. By multiplying the Volt-Ampères by 0.10192, the product is the mechanical work done in the lamp in kilogrm.-meters.

7. *Lamps per Horse-power of Current.*—One horse-power is 75 kilogrm.-meters per second. By dividing 75, therefore, by the number of kilogrm.-meters of work done in the lamp per second, the quotient is the number of such lamps maintained by a horse-power of current.

8. *Candles per Horse-power of Current.*—The number of candle-lights per horse-power of current is obtained, of course, by multiplying the number of lamps per horse power of current by the corrected candle-power of each.

9. *Normal Lamps per Horse-power of Current.*—Conversely, by dividing the number of candles per horse-power of current by the normal value of the lamp in standard candles—in the present case 16 or 32—the number of normal lamps per horse-power of current is obtained.

Summary of Results.

(*a*) *At* 16 *candles.*

	Edison.	Swan.	Lane-Fox.	Maxim.
Candles	15.38	16.61	16.36	15.96
Ohms	137.4	32.78	27.40	41.11
Volts	89.11	47.30	43.63	56.49
Ampères	0.651	1.471	1.593	1.380
Volt-Ampères	57.98	69.24	69.53	78.05
Kilogram-meters,	5.911	7.059	7.089	7.939
Lamps per H.P.	12.73	10.71	10.61	9.48
Candles per H.P.	196.4	177.92	173.58	151.27
Lamps of 16 candles per H.P.	12.28	11.12	10.85	9.45

(*b*) *At* 32 *candles.*

	Edison.	Swan.	Lane-Fox.	Maxim.
Candles	31.11	33.21	32.71	31.93
Ohms	130.03	31.75	26.59	39.60
Volts	98.39	54.21	48.22	62.27
Ampères	0.7585.	1.758.	1.815.	1.578
Volt-Ampères	74.62	94.88	87.65	98.41
Kilogram-meters.	7.604.	9.67	8.936.	10.03
Lamps per H.P.	9.88	7.90	8.47	7.50
Candles per H.P.	307.25	262.49	276.89	239.41
Lamps of 32 candles per H.P.	9.60	8.20	8.65	7.48

VI.—Conclusions.

The following conclusions seem to be sustained by the results which have now been given:—

1st.—The maximum efficiency of incandescent lamps in the present state of the subject, and within the experimental limits of this investigation, cannot be assumed to exceed 300 candle-lights per horse-power of current.

2d.—The economy of all lamps of this kind is greater at high than at low incandescence.

3d.—The economy of light-production is greater in high resistance lamps than in those of low resistance, thus agreeing with the economy of distribution.

4th.—The relative efficiency of the four lamps examined, expressed in Carcel burners of 7.4 spermaceti candles each, produced by one horse-power of current is as follows:—(A). At 16 candles: Edison, 26.5; Swan, 24; Lane-Fox, 23.5; and Maxim, 20.4. (B.) At 32 candles: Edison, 41.5; Lane-Fox, 37.4; Swan, 35.5; and Maxim, 32.4. To double the light given by these lamps, the current-energy was increased—for the Maxim and Lane-Fox lamps, 26 per cent.; for the Edison lamp, 28 per cent.; and for the Swan lamp, 37 per cent.

The contemplated underground railway of Paris is to be 24 miles long including branches and will cost $30,000,000, or $1,250,000 per mile; 10 cents first-class fare, four cents second class fare, two cents workmen's fare, according to the class of the "passengaire."

EXPERIMENTAL MECHANICS.

By OBERLIN SMITH, Bridgeton, N. J.

Transactions of the American Society of Mechanical Engineers.

There is in this country a field of mechanical work, which is of vast importance to its industrial interests, and even to pure science, but which has never been occupied in any systematic way. I refer to experimental mechanics,—the ascertaining by tentative methods the fitness, strengths and qualities of different materials, and their behavior under various strains, motions, processes and continued uses; of their best forms and proportions when worked into parts of machines, and like considerations.

This work has, so far, been chiefly done by individuals, as they felt its absolute need in inventing and developing various machines. Some of it has been done by the National Government, principally to meet its own necessities in naval matters; a little, in the way of testing boilers, etc., to enable it to enforce its steamboat laws. Other portions of the field have been occupied by solitary scientific students and by learned societies, colleges and technical schools, *e. g.*, the Stevens Institute, with its valuable tests of strength and elasticity of metals.

In France there is, I believe, some work of this kind done at government expense, but I have forgotten to just what extent; probably less since she has become a republic than when under the "one man power" régime. In this country we can hardly hope that our government will, in our time, be sufficiently under scientific influence, or alive to the magnificent industrial economy of the expenditure, to devote a few millions to the endowment of a great National University of Experimental Science, with its corps of well-paid professors, selected from the ablest talent of the world, and its thousand earnest students, all at work making records which would speedily be recognized as standards of technical practice.

In default of this the work must be done, as heretofore, by our chemists, and engineers, and mechanics, and electricians. It may be, however, that the time has come for the introduction of more method and system, in order that efforts which are now wasted in needless duplication may be devoted to more accurate *finishing* and *recording* of experiments, and making them accessible to the mechanical public in a properly indexed form. Incomplete experiments are the rule, rather than the exception, when performed by individuals in furtherance of some industrial result. This is simply because the required time and expense deter them from going any further than is absolutely necessary for the case in hand.

Apropos to this part of the subject, I have, in common with others, experienced on numerous occasions the want of a little systematized and "get-at-table" knowledge about some very simple matters. I have, however, always been obliged to fall back upon private experiments, which, in the nature of the case, would have been too expensive if made thorough enough to be of public value.

To select a few instances: Case "A" was regarding common spiral springs—the principles governing their action; the pressure to be obtained with a given motion, with given material, given diameter of coil and wire, and given pitch and number of coils. Nobody knew.

Case "B" was in relation to "drawing" sheet metals, where a flat disk of tin-plate, brass, or other thin metal, is drawn cold into a cylindrical or conical form. Who knows the sizes of these disks to form a given depth and diameter of pan or box? Only those manufacturers who have accumulated hundreds of samples, finding the disk sizes by actual trial (involving ofttimes tiresome alterations to expensive dies) from which they can guess approximately the dimensions for new patterns which they may wish to make.

Case "C" related to permanent magnets. How short could they be in proportion to their thickness? What attractive power had they in proportion to their weight, when magnetized to saturation? What time was required for such saturation with a given hardness of steel, and

a given strength of electrical current in a surrounding helix? Would very minute magnets (say grains of steel dust) behave proportionally as larger ones, etc.?

Case "D" was the simple question: How fast is it safe to run an ordinary grindstone, and what is its bursting speed? A letter to a prominent grindstone manufacturer elicited the reply, that he did not know, but that Messrs. So & So ran their stones so fast, and found it about right. In regard to Case A, I wrote to gentlemen, eminent for scientific research concerning the elasticity of metals, and also to a well-known spring maker. They none of them happened to have studied the properties of springs. In relation to Case C, I consulted one of our most celebrated electricians. It so chanced that he had never specially investigated the properties of permanent magnets, so in all these cases I labored on alone, having also failed to find the desired information by referring to some of the principal mechanical dictionaries, electrical manuals and engineers' handbooks. Perhaps the knowledge searched for is known to somebody, and published somewhere, but it certainly is not readily accessible, as it is in the case of the steam-engine. The latter machine has attained a dignity in the mechanical world that has given it a literature of its own, and all the proportions necessary to a good engine can be found given in detail in printed tables. This is, to some extent, true in regard to cotton machinery, and is beginning to be in plumbing work, and a number of other industries.

It will be seen that the main idea attempted in the foregoing remarks is, that the makers and users of machinery in this country should, for their own pecuniary benefit, as well as for the interest they may feel in applied science, *combine* to establish some sort of a *central council* for experiment and research. The *personel* of this council should include such a number of mathematicians, physicists, engineers and mechanics, all of the highest ability, as would give it the respect and allegiance of the mechanical public. Its *matériel* would be buildings, apparatus, record books and the best attainable scientific library. Its *work* would be: First, the publication and distribution of official information regarding any technical subject which the members should think of sufficient importance, and which might be suggested by themselves, or by any correspondent who needed or desired its investigation; and, second, the fixing of standard sizes and proportions where uniformity of practice is desirable. Its *methods* of work would be literary research, correspondence with practical men, mathematical calculation and mechanical experiment. The latter, however, could in many cases be dispensed with. To collect, compare, average and amplify records of other peoples' experiments and practice would be all sufficient.

A notable instance of such work was the fixing of the excellent "United States Standard," for bolt threads, nuts, and heads a few years ago. It was the combined work of *individuals* (the Messrs. Sellers) for their own practice, a *society* (the Franklin Institute) for the promotion of science, and the United States Government, which latter made it but semi-authoritative by deciding to adopt it in the navy merely.

The important questions arise for consideration, *when*, to *what extent*, and by *by whom*, shall this work be done? To the first, a natural answer is—*now*. The second depends somewhat upon the third, and upon the money and enthusiasm at command. The third answer is respectfully referred to the American Society of Mechanical Engineers, with the hope that, if the subject should seem of sufficient importance, it will be properly discussed. It may be that your learned body, representing the best scientific and mechanical talent of our land, will now or at some future time, see fit to make a beginning in this desirable work. Should such be the case, the possible methods of action are various. A practicable way might be to secure co-operation, and to bring about a systematic division of labor among the societies and schools that are already at work, thus increasing their efficiency many fold. Independent action might be the better method, and, however small the beginning, a nucleus would be formed, around which would, in time, accumulate the intellectual and pecuniary offerings of a grateful and appreciative engineering public.

Should not the engineers of America

to maintain their credit at home as well as the great reputation they have gained abroad, see to bringing about a time when a peregrinating journeyman will not have to master a new system of hieroglyphics upon the drawings at every shop he works in,—when every shop owner will not have to select to suit his fancy from a dozen assorted brands in buying a wire gauge; and figure out twenty different sized pulleys to coax on to his line shafting to drive twenty "eighteen-inch" lathes; and puzzle his brains establishing for himself standard sizes and angles for nut bevels, and machine screws, and key-seats, and loose collars, and drawing-boards; and find in his mechanical dictionary half a dozen speeds, varying some five hundred per cent., as each and all correct for turning cast iron,—when he will not need to build a metal-testing room of his own,—a time, in short, when *one* well-done calculation or experiment shall replace a *thousand* half done, and system shall replace chaos.

DISCUSSION.

The PRESIDENT: I think that matter is a matter worthy of some debate, and a matter of pretty general interest to us all. I presume that after the gentlemen have seen what has been done during the last few months at the Pratt & Whitney Company's works, and have seen what ought to be done at various other establishments that we have visited to-day and at other times, they will come to the conclusion that this work can be systematized by the concerted action of men of adequate knowledge, skill and experience in such a way that the world would be a very great gainer, and that instead of one or two firms expending twenty, thirty, or forty thousand dollars in experiments in getting results that are only of value to them, and of limited value even to them, we should by a proper systematization of methods get for that same expenditure many times the value, and get it in a satisfactory and authoritative form, and in a form that would be accessible and available to all. This proposal, of course, as you all know, is not a novel one. The matter has been proposed before, has been thought of seriously before, I presume, by every man who has had much to do with mechanical work; and it has even taken promising shape on several occasions; but there have always been difficulties in the way, and the result to-day has not been at all satisfactory. Some of the first attempts that have been made to secure practical knowledge by careful and skilfully directed experimentation have been made under the supervision of the government. A committee of the Franklin Institute conducted a series of experiments on the strength of iron many years ago, in connection with the investigation of the cause of steam boiler explosions, that had great value. The results were published in a public document, which is still obtainable, although rare. The results were of great practical value, and remain valuable to-day. Another investigation, made just a little later under the auspices of the government, was that of Professor Johnston on the value of American coal, and that document, containing Johnston's report, on American coal, remains to-day one of the most valuable books an engineer can have in his library.

Mr. WOODBURY: I have tried to obtain that book—is it obtainable? I have asked both booksellers and correspondents and have been unable to get it.

The PRESIDENT: An attempt was made a few years ago only, to institute a series of experiments on the causes of steam-boiler explosions that should be complete, exhaustive and valuable. Congress very liberally appropriated $100,000 and the President was authorized to appoint a board—a commission—which should conduct the investigations. The President was not well acquainted with the men in the country who are capable of conducting such an investigation. There was no Society of Mechanical Engineers to whose officers he could go and of whom he could ask the names of the leading men in the country in the profession, and from whom he might obtain information that should lead to the formation of a proper commission. He did the best thing that he could do under the circumstances, no doubt. In the Treasury Department there exists a bureau, presided over by the Supervising Inspector General of Steamboats. The President made him the chairman of this commission, and appointed a body of men whom he supposed were competent to conduct the investigation and the matter was left in

their hands. They at once proceeded to spend money freely—laid quite large plans; but for causes that need not be mentioned here the expenditure of money was not as wisely made as it might have been. A large proportion of the appropriation was lost from that cause, and after various mishaps—some due to fault and some to misfortune—the board died an unnatural death, leaving their work incomplete. Some work was done—some interesting work was done—but the board has never made a report. The organization changed in form and changed in members. Some distinguished men were on the board at intervals, but the result has been nil. No report exists. Notes were taken by the members of the board, and I presume those notes are in existence. I was on the board for a time until my health failed; and for that and other reasons that were obvious to me I left, and during the period in which I was connected with it, I know the experiments were conducted carefully so far as they went. The notes that were taken I am confident are in existence, and I presume a concerted movement would bring out those notes from those members of the board who are still living, and reports by members to the Treasury Department. If such reports were made, they will be published as a matter of course, and the public document containing reports so given would then become accessible to all. But to-day we can simply look back upon the expenditure of $100,000 nominally to ascertain the causes of steam-boiler explosions, with but little result. If that thing were attempted again, if the same opportunities were offered to-day, I think it is extremely likely that results might be obtained that would be very valuable and more than commensurate with the expenditure. I presume that under similar circumstances the President of the United States and his advisers would look to a body like this Society for advice as to who should be appointed on such a commission and as to what direction to take, perhaps, as to methods of investigation. But the non-success of the board, I have no doubt, has hindered investigation in that direction to such an extent that none of us here present will ever see the matter reopened. I presume the investigation of the causes of steam-boiler explosions, even were it to be considered as necessary as it was thought to be then, will not be again undertaken in a generation.

Fortunately other work, especially of the Hartford Steam Boiler Inspection and Insurance Company, and the works of similar companies in Great Britain, has enabled us to acquire knowledge that could not have been acquired even by such a commission. In the course of their business operations they have been compelled to study up the subject. They have had opportunities of observation and investigation that no government commission even could have obtained; and very fortunately, therefore, as I say, those commercial bodies are acquiring information of great value, and the causes of steam-boiler explosions are gradually becoming known; and I suppose all engineers who have watched the progress of their investigations and studied the results of their work, have come to the conclusion that there are three principal causes of steam-boiler explosions; at least I myself have no hesitation in attributing the great majority of them to three principal causes; the first is ignorance, the second is carelessness, and the third is utter recklessness. Those are *the* three causes of steam-boiler explosions. The number of steam-boiler explosions of which the causes remain unascertained is a very small percentage of the total number, perhaps four or five per cent. I do not know what the figure is precisely, but it is very small, and those are principally cases where lack of knowledge comes simply from lack of opportunities of observation. So that it may be stated as a positive fact, I can say, that we know to-day, that steam-boiler explosions can be attributed simply to easily preventible causes, and the work of such a commission is not to-day as much needed as it formerly was. It remains possible that there are causes of steam-boiler explosions which are very rarely operative and which still remain undetermined, perhaps unsuspected; but they are so rare, that they have no direct value—no direct importance, I should say. Another attempt was made a little later to make a serious of investigations under the auspices of the government, which resulted more favorably, but still not as favorably as we might wish. A

committee of the American Society of Civil Engineers first took action several years ago—I think it must be ten years ago now—toward the creation of a government commission to investigate the strength of American materials. They have a standing committee—you will find their names printed on every issue of the Transactions of the Society, on the first inside page of the cover—a standing committee on the tests of American iron and steel. The object of that committee was to secure the appointment of a commission and the inauguration of an investigation, such as Mr. Smith has suggested here to-night.

After some years of somewhat ineffective work, their efforts were finally successful, and Congress directed the President to appoint a board to make tests of iron and steel, and other metal, and to report results. That board was to consist of an engineer officer of the army, an engineer officer of the navy, an ordnance officer of the army, and three civilians. This board, so constituted of persons who were expected to be experts in the direction that the investigations were to take, was appointed by the President accordingly, and Congress made an appropriation of $75,000 to do this work, with a proviso, as the bill first was passed through the house, that $15,000 should be used for the expenses of the board, and that $60,000 should be appropriated to the construction of a machine. In the meantime the Committee of the Society of Civil Engineers, who had been acting energetically with the appropriation committee to secure the appointment of the board, found that some influence was at work that they had not known anything of, and that influence had secured this peculiar wording of this resolution which was to be a joint resolution of both houses; but, by their action, and, possibly, by the action of friends unknown to them, the wording was finally changed, and an appropriation was made of $75,000, which was to be used at the option of the board in their work. Part of the wording still remained as before; that is, they were allowed the use of $15,000 for the commission. The interpretation naturally given to that was that it was to be used in paying expenses of the commission, traveling expenses and incidental expenses. The board met immediately after its appointment at the Watertown Arsenal, and received there, at a subsequent day, plans for the construction of testing machines, with specifications and prices that were named. They selected a plan which seemed to them the best, directed the construction of such a machine, and appropriated the required amount of money for it. The contract called for an expenditure of $31,500 on the machine. They were informed that the chief of ordnance (as this machine was to be placed at the Watertown Arsenal, and would fall into the hands of the Ordnance Bureau when the board had completed its work), would put in the foundations of the machine, and thus save the board a considerable amount of expense. But that was not stated officially and ultimately; those foundations were put in at the expense of the board, so that the major part of the appropriation of the first year was expended in the construction of a testing machine.

But, while waiting for the construction of this testing machine, which was intended for the testing of very large masses of iron and steel, the board went into subsidiary investigations, as they considered them, intending to make the more important investigations,—the investigations into the strength of structures and large masses of iron and steel,—after that machine was completed; and, so long as that appropriation remained in hand, they continued their work there, and they expended the full amount of the appropriation upon the machines, or upon these investigations. The amount used in the personal expenses of the board amounted to very little. The members did their work as best they could, and at an expense that was insignificant, outside of actual cost of making tests. The result of the work of the board, so far as it was carried out, was published in a public document in 1878. That document can be found by members during the coming summer at Washington, and I believe it can be procured by application to your representatives. But the appropriation, of course, was soon exhausted, and Congress gave another small appropriation the succeeding year.

But after the machine was completed, and after these investigations were well under way, and the board was just in

good condition, in every respect, to go on and do work that should be creditable and valuable, Congress declined to make any appropriation, even for the use of the machine that they had built, and the board died in consequence of the expiration of its appropriation. The limit of life for the board was fixed by the limit of its appropriation. When the appropriation expired the board ceased to exist. So the board went out of existence just when it was getting ready to do its work, and to do good work; what it could have done gentlemen can judge very well by reading the report which will be published this summer. In that report you will find what was done with about fifteen or twenty thousand dollars. The financial statement is in the report, and you can judge for yourselves how much that work is worth, and how well the expenditure of the board has been repaid by the acquisition of knowledge. But Congress seemed to have no appreciation of the importance of that work and declined to do anything for the board. An immense amount of influence was brought to bear upon the appropriation committees, but without the slightest effect. Memorials were sent in by the American Society of Civil Engineers; by the Society of Mining Engineers; by the iron and steel associations; by the faculties of all the prominent technical schools; by the faculties of some of the best known colleges; and recommendations were made by a large number of well-known business men, and influence brought to bear upon the appropriation committees by members of Congress from all parts of the Union. Some gentlemen worked very earnestly, and yet an amount of influence that would naturally and ordinarily secure the appropriation of almost any amount of money, and carry through Congress any reasonable,—any at all reasonable,—proposal, failed to secure another dollar of appropriation for the board.

The machine, when completed, came into the hands of the Ordnance Bureau of the army, and is now in use by them doing good work. An appropriation was secured by the Ordnance Bureau, at the last session of Congress for the continuance of work with that machine, and there seemed to have been no difficulty in securing that appropriation, but the influence of all the business men in the country, the influence of all the scientific associations in the country, the influence of all the faculties of the technical colleges in the country combined, could not succeed in getting the appropriation. So that gentlemen can see what is to be done if they expect to accomplish anything further in that direction. So long as the interests of the community seem to lie in the direction of the production of a testing machine simply, there was no difficulty. When it seemed likely that the board would be able to use that machine effectively, there was difficulty; and I presume the conditions remain to-day as they were then. Those are the ways in which attempts have been made; and I have indicated about how much success has been met with in the way of securing effective scientific work that would be valuable to the business men of the country, under the general administration of the government. If the attempt is made to secure such work outside of the executive departments of the government, you will find the difficulty still greater. Members of Congress do not like to put money into the hands of irresponsible parties. It is much easier to get money appropriated for use by a department of the government than for any work to be done outside; and the only chance in this case was to secure the co-operation of the government officials with civil appointees.

I am taking a great deal of time, but I would like to say a few words about some other work that has been attempted. If the gentlemen will bear with me I will go on for a few minutes longer.

Several MEMBERS: Go on.

The PRESIDENT: A few years ago two or three prominent gentlemen connected with our railroads came to me and asked if some such commission could not be found, if some such method of doing work could not be inaugurated; or if we, at the Stevens Institute of Technology, at Hoboken, could not ourselves start in a small way some such investigations as have been called for in the paper just read. I saw no reason why it should not be done, and told the gentlemen if they would give us the necessary capital and allow us time to do our work well, that we would accomplish anything in that direction, and I myself had no ob-

jection at all to making the attempt. I saw the trustees and they naturally were very glad indeed to lend a hand in the matter, and the matter seemed to have been agitated in various directions. Members of the Society of Civil Engineers spoke of it, and took official action in the matter in their meetings; and a good many individuals at about that time seemed to have taken very much interest in the subject. That focused the movement at the Institute, and inaugurated what we called the Mechanical Laboratory of the Stevens Institute of Technology. I had no funds, I had no assistants, I had nothing but the countenance and the interest of these gentlemen. But I proclaimed that we would establish a Mechanical Laboratory at the Stevens Institute of Technology, and went ahead. Fortunately, at this time, the government board had just been instituted, the commission of which I have just spoken; and as chairman of some of the committees of that board, I was directed to make certain investigations. I simply took the apparatus of the Stevens Institute of Technology, and for a time appropriated it to the use of the board; found some bright young men who had gone through the course, had graduated creditably, and shown themselves skilled in manipulation, and put them at work; and with, of course a good deal of supervision on my part, but with active, earnest work on theirs. we succeeded in doing a large part of the work that actually was done by the government commission. A good deal of work was done outside. Mr. Holley did a good deal; General Smith did some. A large amount of very valuable work was done by a committee consisting of Commander Beardsley and some other gentlemen, in the investigation of the properties of iron; our Mechanical Laboratory took charge of a certain amount of that work, and that was a starting-point.

I borrowed money where I could, and I begged money where I could; and where I could not do either, I took it out of my own pocket. But in various ways I accumulated apparatus and testing machinery, and set going the Mechanical Laboratory of the Stevens Institute of Technology. Well, the amount of work done there amounts to-day to about $40,000 worth of experimental work. That is direct scientific investigation, and directly in the line that is indicated as desirable in the paper that has been read. But my duties and the work that I had accepted from outside professional practice, proved to be too much of a load for me, and I broke down; and during my absence from the Institute the work done by the laboratory naturally became less and less. My colleagues took a very earnest interest in what was going on, and much work was still done; but the amount of work became gradually less and less, until on my return I found very little was being done, almost nothing, in the direction of investigation; and since I have been back I have not had the strength or time to push the experiments as I did at first. We are now doing a small amount of commercial work, making examinations of the strength of materials for the Dock Department of New York; the Erie Railway, and private parties in all parts of the country. But it is purely commercial work. It does not lead up to what Mr. Smith asks for; the scientific determination of laws and facts in such form as to be accessible to the public. And I am not very certain that as matters go now I can re-establish that adjunct to my department on the basis that I had hoped to put it upon. If I get strength, and if friends assist us in an interested, active, earnest way, I have no doubt we could find funds enough to endow it. But it requires work; and one man, I find, cannot do more than about three men's work. Consequently the success of such a scheme depends, you see, not only on the interest of the members of the profession, but on the activity that that interest inspires. The whole thing is perfectly feasible. The plan of making such investigations in the manner which is always expected in scientific work can be carried out. It simply requires brain, physical strength and capital; and if the Society can find a way of bringing those things together it will accomplish results that will be simply wonderful.

Mr. HOLLEY: I would like to add one word, Mr. President, to what you have said. I could say a good deal upon the subject, but the time is passing rapidly. It must be obvious to the Society that the Ordnance Department of the United

States Army does not wish to co-operate with that perfect harmony with civilians that might, under some other circumstances, have been expected, not to put it too strongly. Seeing that the Ordnance Department may not wish to go into that co-operation with civilians in conducting these experiments, but that it desires to control that matter itself, if that is the only way in which it can be made to help us in this work, then, certainly, it becomes the duty of the mechanical engineers to try to stimulate the Ordnance Department to make experiments that will be useful to us and the industrial arts generally, and not useful merely to the Ordnance Department. I just throw out that mere hint.

The PRESIDENT: And I would add to my remarks on the work of the United States commission appointed to test iron and steel, that the discovery by the president of the board of the inventor of that testing machine, Mr. Albert Emery, is enough of itself to justify the creation of that board, and the expenditure of all its money. I think the discovering of Mr. Emery was one of the greatest discoveries of the age; and the construction of the testing machine has been one of the greatest pieces of engineering work that ever has been done. That machine has done and it is doing its work; and if nothing more has been done by the board, as I said a moment ago, that is a great deal, fully enough to justify the creation of that board, and the expenditure of all the money that has been and will be expended upon that machine. The machine is open to the use of the public, and it is being used to-day very largely, and is in almost constant use by our business men. And I would say, too, that although I do not feel at all satisfied with the results of my experiments in the establishment of a mechanical laboratory, I think that our success, so far as we have obtained results, has been quite sufficient to repay all the expenditure of time, health, energy, strength and money that has been made on it.

Mr. STIRLING: I would like to call the attention of the gentlemen to another way in which we can get the information, to some extent, that has been asked for. Having the good fortune to be a lieutenant of Mr. Eckley B. Coxe, of Pennsylvania, I have the privilege of being under the same roof with one of the finest technical libraries in the country; and in that library we have a book which is published by the German government, —I do not know of what bureau in that government, and that book gives a statement of every article that is published on every subject in every country. And as an illustration of what good this is to us, the other day I had occasion to look up the subject of the transmission of power by friction gearing. I asked the librarian to give me all the literature there was on the subject, and I got a list of thirty or forty articles, published in different languages, on the subject of the transmission of power by friction gearing. I think that in that way gentlemen can be posted upon a great deal of this experimenting that has been done by individuals, on almost every subject.

Mr. SMITH: I would like to say a word more, if it will not take too much time; as this is a subject on which I feel very deeply. I feel that I am too young a member of the Society to make a motion on the subject, and shall not do it to-night. But I think that a committee should be appointed to consider the question, and report at a future meeting, whether anything can be done by this Society, or whether the matter should be left entirely alone. If, however, anybody here wants to make a motion I shall be very glad. What is wanted is not only the ability to get at the technical books and articles that have been published on the subject, but a brief *résumé* of them. An average manufacturer cannot afford to search through a half dozen learned books, even if he can get them, and collect all the information that is given there and condense it. He wants to be able to correspond with a standing committee of this association, or some other that is known as a standard throughout the country, and get at the best figures, which need not be exactly accurate, something just to guide him so that he will not go too far astray on any particular thing he is working on. It is useless to hope, as our President says, for much money to be spent by the government; still we can all do what we can in that direction, by bringing it before Congress and friends who have influence there. Whatever is gained will be gained by independent work;

and although it may not be much now, on account of the want of means in this Society, yet the Society will grow and we will get more means, and this expense might, perhaps, be paid by the members. It would not be a very great expense to keep up an organization with which people could correspond and which would give the results of what has been done. After a while it would grow to be of such importance, that it would be a standard for working from by all progressive men. And, something I did not mention in the paper, that is wanted greatly among our mechanics is a standard of nomenclature. Great confusion results now from having half a dozen names in different machine shops for the same thing. That, and standard sizes of gauges, and the collection of needed information, and the answering of questions regarding what has already been done, would not be such an immense work, and could be done at comparatively small cost. Although I do not think the Society is large enough to undertake it now, yet we can all use our utmost endeavors to make the Society grow, get membership of the right kind, more money in the treasury, and after awhile we shall see the importance of this subject so clearly as to be willing to spend a little of our money. I shall, certainly, at another meeting bring about some kind of a motion for a preliminary committee to investigate the subject more at length, if it is not done now by somebody.

The PRESIDENT: The accomplishment of anything in that direction will require a great deal of careful thought, preliminary work, and cautious procedure. It involves a good deal more than gentlemen generally are disposed to anticipate. It means the devotion of some man or men exclusively to a certain object; and if a manufacturer cannot afford to give the time to the looking up of a half a dozen references, it is doubtful if he can find any other man to give his time to looking up a hundred references for a hundred different persons. To get good work done requires the expenditure of a good deal of money; but it is a matter that has been deemed of sufficient importance to be called to the attention of other leading societies in the country, all the technical societies and faculties of technical schools have considered it as of great importance; and I have no doubt that with a special and concerted action, the time will come when the thing will be established. Referring to Mr. Stirling's remarks, the work he refers to is *Carl's Repertorium*, and it was published for quite a long series of years in Germany, by the editor Carl; and he was succeeded by Schubarth, so that the late issues are called "*Schubarth's Repertorium*." Gentlemen interested in investigations who wish to look up references, by obtaining a set of that work, will put themselves on the track of about all that has been done in the direction of scientific and technical research. And then in reference to what has been done in this country, turn to the files of the *Journal of the Franklin Institute*. I do not know how many volumes of that have been published, perhaps sixty or eighty volumes, but it runs back a great many years, and contains an account of almost all the important work that has been done in this country. The *Philosophical Magazine* gives an account of the greater part of the valuable scientific work done in Great Britain. The *Annales de Chemie et de Physique* tells you what has been done in France; and you will find if you go to the Astor Library, in New York, that the librarians can always put you exactly on the track of what you need if it is published at all. *London Engineering* is to the engineer a perfect mine, and a mine you will never tire of working.

M. CAILLETET has invented a new pump for compressing gases to a high degree of compression. The main point in its construction is the method by which he obviates the existence of useless space between the end of the piston-plunger and the valve, which closes the end of the cylinder. This he accomplishes, *Nature* says, by inverting the cylinder and covering the end of the plunger with a considerable quantity of mercury. This liquid piston can of course adapt itself to all the inequalities of form of the interior space, and sweeps up every portion of the gas, and presses it up a conical passage into the valve. The valve by which the air enters the body of the pump is opened by cam-gearing after the descent of the piston below the point where the air rushes in.

PILE-DRIVING FORMULÆ.*

By A. C. HURTZIG, Assoc. M.I. C.E.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

IN an article on Pile Foundations in VAN NOSTRAND'S ENGINEERING MAGAZINE for July, reference is made to a "Note on the Friction of Timber Piles in Clay," by the author. In addition to this note, he on a former occasion investigated the subject of pile-driving, with the object of obtaining a simple and practical method of determining the relations between weight of ram, fall, "set" per blow, and supporting power of any pile. This result when applied to the experimental pile driven at Proctorsville, gave a supporting power almost the same as the actual load that was found necessary to move the pile.

The inquiry led to the construction of a set of diagrams, from which by mere scaling, any particular condition of pile driving could be obtained when the other conditions were known. The use of diagrams always commends itself to the practical engineer who has generally no inclination to wade through tedious formulæ and figures with great risk of error, when he can obtain the information he requires in a shorter time and with small chance of error. In this article is given the reasoning by which the results were deduced, and the author claims for these formulæ and diagrams that they are based on exact scientific principles, and that since the constants are determined from a large series of experiments, they are practically reliable.

In the July number of this magazine before referred to, a comparison was made between twenty recognized formulæ, and an actual experiment on a pile at Proctorsville. As was pointed out, the discrepancies between the two results are truly remarkable, and none of the formulæ go very near the actual facts. The main particulars of the experiment were as follows: (See July number page 23).

Length of pile=30 ft.

Scantling, $12\frac{1}{2}'' \times 1''$ at top. $11\frac{1}{2}'' \times 11''$ at bottom.

Weight of ram=910 lbs.

Fall of last blows=5 ft.

"Set" at last blow=$\frac{3}{8}$ inch.

With these conditions, the author's formula, as will be presently shown, is

$$Y=\frac{x}{P}-\frac{P}{625},$$

in which

Y="set" of last blow in feet=.03.

X=energy of do. do. in foot-tons=

$$\frac{5\times 910}{2240}=2.031.$$

P=extreme supporting power of pile in tons. Inserting these numerical values and transposing, the equation becomes

$$P^2+18.75P-1269=0,$$

whence

$$P=27.47 \text{ tons}=61{,}533 \text{ lbs.}$$

The actual load which caused motion in the pile was 62,500 lbs., so that the formula gives a close approximation.

In arriving at this result, the author considered three formulæ which are probably more relied than on any others.

These were

Rankine's,

$$P=\sqrt{\left(\frac{4.E.S.W.h}{l}+\frac{4E^2.S^2.y^2}{l^2}\right)}-\frac{2ES.T}{l}$$

Sanders,

$$P=\frac{W.h}{8.y}$$

McAlpine,

$$P=80(W+0.228\sqrt{h}-1)$$

in which the letters have the following significations:

P=weight to be supported. } measured in the same unit.
W=weight of ram. }

h=height of fall. } measured in the same unit
l=length of pile. }
y=depth driven by last blow. }
S=sectional area of the pile, (to any unit).

* Part of this article is an abstract of a Paper read by the author at a supplemental meeting of the Students of the Institution of Civil Engineers, London.

E=modulus of elasticity of the timber referred to the same units as W. and S.

Rankine's formula is purely theoretical, and though expressing the true relations between the quantities, it fails as a practical formula in that its contents are not derived from experiment on such a scale as would justify their use in every-day pile-driving practice. Thus in this formula the modulus of elasticity has a value deduced from the elementary experiments on the strength of materials. Pile heads under the process of driving are by no means comparable with the perfect specimens of timber used in laboratory experiments; yet no allowance is made in the formula for this fact. It is also so cumbersome, as to render its use difficult and distasteful to the practical engineer.

By putting $y=0$ it follows that

$$P^2 = W.h.\frac{4E.S.}{l},$$

whence it appears that the supporting power of a pile is proportional to the square root of the fall. Now the formula of Major Sanders gives the supporting power as proportional to the first power of the fall, and this relation is evidently an incorrect one. Sanders' expression was deduced from experiments, and may be trustworthy within a certain small range of conditions corresponding with those of the experiments. It is probably admissible when there is a considerable "set" per blow. In cases of small "set" it gives excessively high results, and in the limiting case where $y=0$ the pile would support an infinitely great load, no matter what weight of ram he used, a a result the fallacy of which is evident.

McAlpine's formula is of much value, as having its constants deduced from a large number of piles, and the form of his expression, as will be shown immediately, is the same as Rankine's in a particular case. McAlpine recommends his result only between certain limits, and these restrictions render the formula inapplicable in a great majority of cases. For instance, it is recommended for falls between 20 ft. and 40 ft.—limits between which but few piles are driven—in England at least. There is no reason, however, why McAlpine's experiments should not be used as a special case for determining the constants for Rankine's general formula.

Rankine's original expression is this:

$$Wh=\frac{P^2l}{4ES}+Py \quad . \quad . \quad . \quad . \quad (i.)$$

in which the total energy ($W.h$) of the blow is represented as having been destroyed by two processes, viz.: the compression $\left(\frac{P^2l}{4ES}\right)$ of the pile, and the energy ($P.y$) required to drive the pile through a distance y. A considerable modification is necessary in this owing to various disturbing influences which are omitted, but which from their variable and indefinite nature must necessarily be omitted from a general theoretical investigation. Firstly, there is the friction of the leaders, and the atmospheric resistance. McAlpine found that a 1 ton ram falling from a greater height than 40 ft., will not even in a very well constructed pile engine attain to a greater velocity than if it fell from 40 feet only. This is contrary to the indications of theory, and it is such discrepancies as this which have to be met in a theoretical formula by suitable coefficients. In the next place, as the ram reaches the pile head in each successive blow, it meets with a material the elasticity of which is different from what it was before, owing to the destruction or modification of the elastic properties of some or all of the fibers in the pile head. This effect on the compression of the timber, the correct representation of which will elude all theoretical inquiry, must again be represented in the formula by some constant derived from extended experiments. Lastly, there are certain irregularities in the nature of the surface of the pile, in the verticality of the driving, &c., which will still further modify the formula. The remaining energy of the blow is absorbed in compressing the timber and imparting motion to the pile. In Rankine's expression (i) above, the motion of the pile enters the last term only, and it is only the first term that will require modification on account of the various disturbing influences enumerated. For suppose the pile going $\frac{1}{10}$ inch per blow or some other extremely small amount, and suppose at the next blow it refuses to go at all. The disturbing influence, in the last cases where the

pile is in a state just bordering on motion, must be exactly the same as they were when the pile just moved, and they must consequently appear in the formula in the limiting case. But then the second term $(P+y)$ vanishes since $y=o$; hence the disturbing influences must be represented in the first term, and the expression in the limiting case will take this form:

$$W.h=C.\frac{P^2l}{4ES} \quad . \quad . \quad . \quad . \quad (ii.)$$

where C is mere constant.

This case corresponds with the conditions of McAlpine's experiments, and from these experiments the vulue of C may be obtained with a considerable degree of accuracy, since observations on as many as 7,000 different piles were taken. To compare McAlpine's formula with (ii) above write W=unity, and $\frac{1}{k^2}$ for $\frac{Cl}{4ES}$ which will be a constant quantity for any particular pile; (ii) then becomes by transforming

$$P=k\sqrt{h} \quad . \quad . \quad . \quad . \quad (iii.)$$

while McAlpine's formula becomes

$$P=18.24\sqrt{h} \quad . \quad . \quad . \quad (iv.)$$

and these two results are quite similar. The numerical conditions of McAlpine's experiments are briefly these:

The average driven length of the piles was 32 ft. Allowing for re-heading, &c., this is equivalent probably to an average length of about 36 ft. while driving. The piles were round straight spruce spars of an average diameter of 11 inches, or sectional area of 96 sq. inches.

The ram was 1 ton falling through 30 feet. The average distance driven in the last five blows was 1 inch, the last blow of the ram driving the pile *nil*.

The actual weight—found from many cases—to move a pile so driven was 100 tons each pile.

By inserting in (iii) these numerical values it reduces to

$$P=\frac{86.4}{\sqrt{c}}\cdot\sqrt{h}$$

and by comparing this with (iv) it appears that

$$\frac{86.4}{\sqrt{c}}=18.24$$

$$\therefore\sqrt{c}=4.74 \text{ and } c=22.4.$$

Having found the value of c, Rankine's expression will now be

$$Wh=5.6\frac{l}{ES}P^2+Py \quad . \quad . \quad . \quad (I).$$

This formula will be applicable in any ground; for since a pile resists motion by virtue of lateral compression, the depth driven by the blow is *cæteris paribus*, the exact indication of the nature of the ground. It can be no matter at all what is really its mineralogical character, except in its effect in opposing the motion of the pile, and this effect is known by the measurement of the depth driven by the blow, or the "set." The very substitution in the formula of the numerical value of this "set" at once renders it applicable to the particular ground in which the pile is being driven.

Referring now to formula (I), the value of the ratio of the length in feet to the sectional area in square inches $\left(\frac{l}{s}\right)$, in practice varies generally between the limits $\frac{1}{4}$ and $\frac{1}{8}$. Where round spars are driven the ratio may be increased to $\frac{1}{2}$, but this is an unusual case. The average value of E, the modulus of elasticity for timber of the fir and pine classes, such as are commonly used in pile-driving, is 700 tons. Making use of this value of E and taking values of $\left(\frac{l}{s}\right)=\frac{1}{4}$, $\frac{1}{5}$, $\frac{1}{6}$, and $\frac{1}{8}$, the numerical equivalents of $\frac{5.6\times l}{ES}$ are respectively $\frac{1}{500}$, $\frac{1}{625}$, $\frac{1}{750}$ and $\frac{1}{1000}$. Representing now the energy of the blow $(W.h)$ by x, inserting the above numerical quantities and transforming, a series of four formulæ is obtained as in the following table (see next page).

These formulæ give rise to two sets of diagrams. For a description of their construction and use one case will be taken. The annexed figures refer to the first formula in the table.

$$y=\frac{x}{P}-\frac{P}{500}$$

Taking x and y as variables in this, it is the equation to a straight line cutting the axis of x at a point $x=\frac{P^2}{500}$. The ordinate y of this straight line at any point gives the set per blow corresponding to the energy x at that point. For different

Formula.	Ratio $\frac{l}{s}$	Corresponding lengths of Pile for Sectional Area 12 in. × 12 in. = 144 sq. in.	Sectional Area 13 in. × 13 in. = 169 sq. in.	Sectional Area 14 in. × 14 in. = 196 sq. in.
		ft.	ft.	ft.
$y=\frac{x}{P}-\frac{P}{500}$	$\frac{1}{4}$	36	42	49
$y=\frac{x}{P}-\frac{P}{625}$	$\frac{1}{5}$	29	34	39
$y=\frac{x}{P}-\frac{P}{750}$	$\frac{1}{6}$	24	28	33
$y=\frac{x}{P}-\frac{P}{1,000}$	$\frac{1}{8}$	18	21	24

arbitrary values of P there are different straight lines as shown in Fig. 1. To illustrate the use of this diagram, let it be required to determine the conditions of driving for a pile which shall sustain a weight of 20 tons. Taking 3 as a coefficient of safety, the line P=60 tons will be the one to consider. This line cuts the axis at a point where $x=7.2$. Here then $y=\text{set}=o$, that is to say, a ram of one ton falling 7.2 ft., and driving the pile till it refuses to move, will be sufficient to enable the pile to carry a load of 20 tons, and for this particular case 7.2 ft. is the least fall that can be used. If it be desired to use a 12 ft. fall the energy of blow $x=12\times1=12$ foot tons. The corresponding value of $y=.08$ ft., or "set" per blow=1 inch; and if the driving has been regularly diminishing down to this point it may cease, and the pile will safely sustain the required load.

If now in the formula, y is taken as arbitrary, while x and P are the variables, the equation is one to a parabola and the curves drawn in Fig. 2 represent these parabolas for different arbitrary values of the set "set" y. The ordinate to the curve for any particular "set" per blow gives the extreme supporting power corresponding to the energy x at that point. For example, required the extreme supporting power of a pile driven by a one ton ram falling 12 ft. until the "set" per blow does not exceed $\frac{1}{2}$ in.=.04 feet. The ordinate, for $x=12$, to the curve corresponding to this "set" of .04 ft. is 68, and 68 tons is the extreme value required.

The remaining case where y and P are variables x being arbitrary is not of so much value, since all possible information required is given in Figs. 1 and 2 for the particular given conditions of any machine.

With regard to the experimental pile at Proctorsville, the value of

$$\frac{l}{s}=\frac{30}{12\times12\frac{1}{2}}=\frac{1}{5}$$

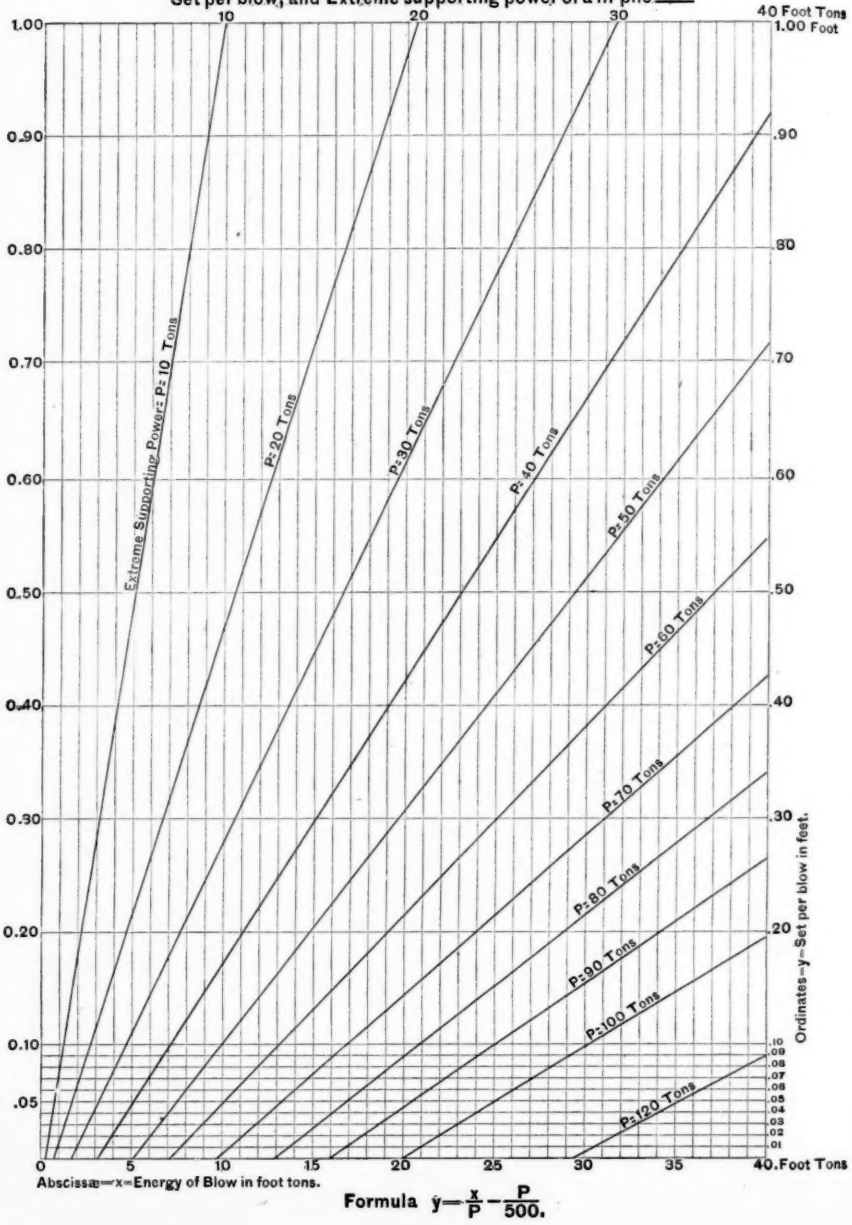
and the formula to be used is

$$y=\frac{x}{P}-\frac{P}{625}.$$

Factor of Safety. As pointed out above, the particular ground in which a pile is being driven, so far as its resisting power is concerned, is always taken into consideration by the mere insertion of the "set" in the formula. If two piles be driven to the same resistance, but in different soils, there is little doubt in the author's opinion that these two piles would sustain nearly equal dead weights. In sandy soils, after a lapse of time, no doubt the resistance to driving increases, and therefore the supporting power of a pile would also generally increase. In clayey soils probably this improvement takes place only to a very slight extent. The great use of a factor of safety is to cover the irregularities which occur on a work, and which are not anticipated or provided for from the office. A contractor for example does not always carry out the work to the letter of the specification, and a pile ordered to be driven to a certain resistance under a certain blow, may be left in a very different state from what was intended. Again, the formula is taken to apply to a pile the head of which is in fairly good condition, but though a pile head may be battered almost to a pulp, it is often thought by foremen pile-drivers not worth while to re-head it if it is going say $\frac{1}{2}$ inch when a specification may require $\frac{1}{4}$ inch. It is considered sufficiently near, and is left as driven. But this idea—perhaps pardonable in an ignorant workman—involves a great reduction in the supporting form of the pile. The author has seen a spongy-headed pile driven until it refuses to go; after being re-headed the pile has under the same fall gone $\frac{3}{4}$ of an inch. Such a thing repeatedly occurs. On any small job where one pile engine is used, it is a simple

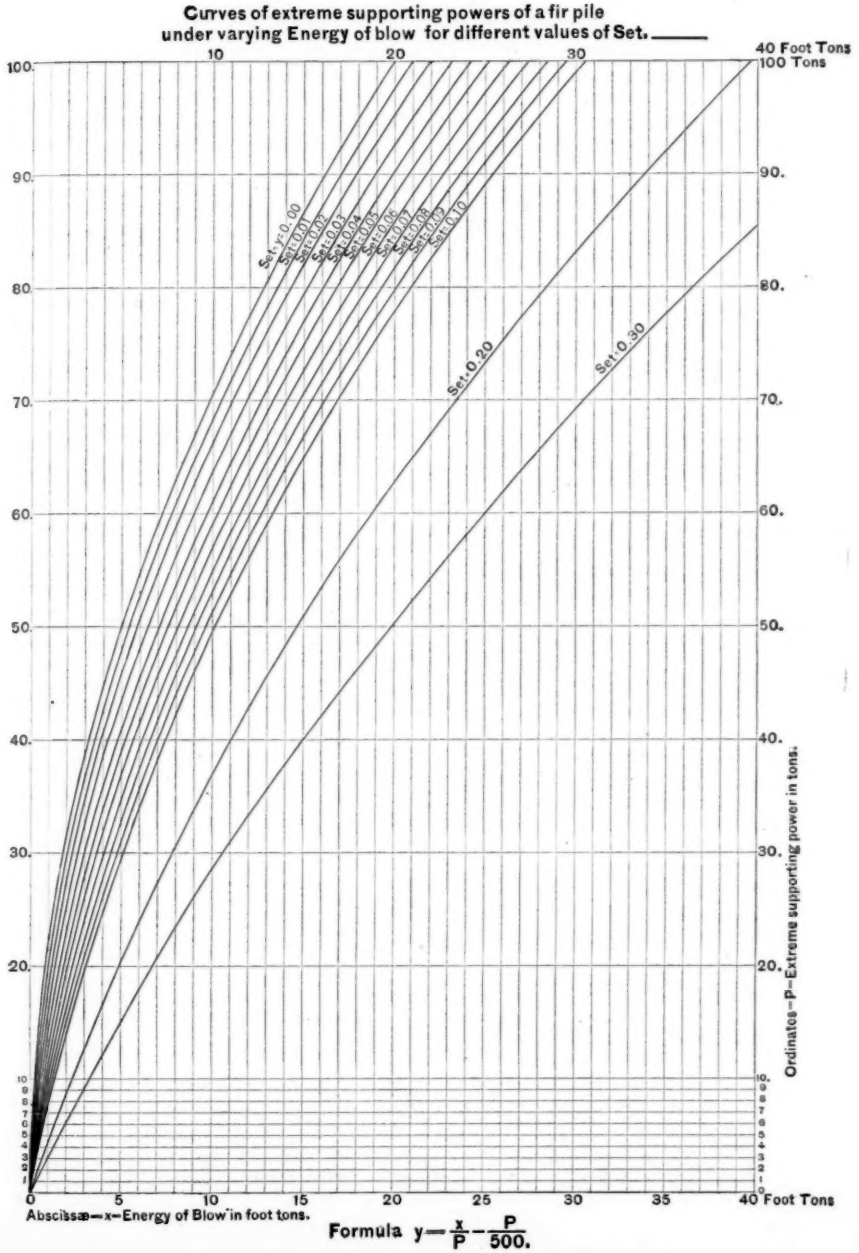
Fig. 1.

Diagram showing relation between Energy of blow, Set per blow, and Extreme supporting power of a fir pile.



Formula $y = \frac{x}{P} - \frac{P}{500}$.

Fig. 2.


Curves of extreme supporting powers of a fir pile under varying Energy of blow for different values of Set.
40 Foot Tons
100 Tons
Set: 0.00
Set: 0.01
Set: 0.02
Set: 0.03
Set: 0.04
Set: 0.05
Set: 0.06
Set: 0.07
Set: 0.08
Set: 0.09
Set: 0.10
Set: 0.20
Set: 0.30
Ordinates—P—Extreme supporting power in tons.
Abscissæ—x—Energy of Blow in foot tons.
40 Foot Tons
Formula $y = \frac{x}{P} - \frac{P}{500}$.

matter to ensure each pile being driven correctly, but on a large work such as the author is in charge of, where 15 steam pile engines are in use and where some thousands of piles have been driven, it is certain that a large number will escape inspection. Then again, here and there, a stick of timber may get driven of poorer quality than the surrounding piles, and after a short time this pile may become useless for supporting the superincumbent structure.

In the course of years it is probable that data may be obtained, comparing actual dead weight resistances in different soils with the indications of some theoretical formula, but there will still remain the necessity for an arbitrary factor of safety which will in the judgment of the engineer suit the particular case in question. What considerations should determine the value of this factor? There are no means of determining the numerical equivalents of such irregularities as are named above, except a comparison with records of actual works executed. By a consideration of such works in Europe the author concludes that with ordinary piling engines giving from one to six blows per minute, a factor of safety varying from $2\frac{1}{2}$ to 5 will include the range of ordinary practice. Now as far as crushing of the timber is concerned, a 30 ft. pile 12 inches square will safely carry 50 tons, and as the safe load on a pile is very rarely if ever made equal to this, the factor of safety for driving will not interfere with that for crushing. The factor deduced—$2\frac{1}{2}$ to 5—will then not be too low to meet contingencies, and as these are the numbers that recommend themselves by a comparison with recent practice, the author would adopt them as the limits for use with the diagrams. The number 3 is sufficiently high for most cases.

In regard to piling engines of the Nasmyth type delivering blows up to a rate of 60 a minute, experiments have been made which show that a given energy expended by such an engine in blows delivered in rapid succession would do $2\frac{1}{2}$ times the amount of effective work that could be accomplished by an equal energy from a hand engine when the blows follow each other slowly. From this, and from a comparison with recent works, it is probable that the diagrams or formulæ would give tolerably accurate results for the Nasmyth type of pile-driver if the factor of safety taken were between the limits 1 and 2.

HOUSE DRAINAGE AND SANITARY PLUMBING.

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II.

ESSENTIAL ELEMENTS OF A SYSTEM OF PLUMBING.

WE have thus far considered only the material, size, general arrangement and manner of jointing the drain, soil and waste pipes in a house. We must now consider what the essentials of the system are, in order to secure to the house perfect immunity from sewer gas. Briefly stated, these essentials are as follows:

1. *Extension of all soil and waste pipes through and above the roof.*

2. *Providing a fresh air inlet in the drain at the foot of the soil and waste pipe system.*

3. *Trapping the main drain outside of the fresh air inlet, in order entirely to exclude the sewer air from the house.*

4. *Providing each fixture, as near as possible to it, with a suitable trap.*

5. *Providing vent pipes to such traps under fixtures as are liable to be emptied by siphonage.*

EXTENSION OF SOIL AND WASTE PIPES.

The *first* requirement asks for a vertical extension of all soil and waste pipes through the roof. This extension affords a ready outlet for all gases that would otherwise tend to accumulate inside the pipe system. In the case of soil pipes nothing short of an extension the *full bore* of the pipe will answer this purpose.

It has been proposed, of late, to enlarge the soil pipe from the highest floor to the roof to six inches diameter, in order completely to prevent any stagnation of air in the pipe. Waste pipes should be enlarged from the point where they pass through the roof, to four inches diameter, as smaller outlets are liable, in cold climates to become obstructed by the freezing of condensed vapor. Plumbers sometimes use galvanized wrought iron or tin pipes for this extension, but this is decidedly bad practice. It should be of the same material as the main soil pipe, and its joints should be worked with equal care.

The extension of soil and waste pipes should terminate at a distance from any windows, louvred skylights, or ventilating flues, and at least two feet below the top of the nearest chimney. It is desirable to have this extension as high as possible above the roof, so as well to expose the mouth of pipe to the influence of air currents. In order to prevent any obstruction of the soil pipe, plumbers often cover the mouth with a return bend. This, however, is objectionable, as it interferes with proper ventilation. Less bad is the plan of capping the soil pipe with a suitable fixed cowl, such as, for instance, Emerson's or Wolpert's ventilator. The best plan seems to be to do away entirely with any cover to the soil-pipe mouth. Capt. Douglas Galton, in his book "Construction of Healthy Dwellings," says in regard to this question: "A tube or shaft with an open top acts best. It is, however, necessary to protect the top to prevent rain from entering the tube; but a cover tends more or less, according to its shape, to delay the current in the tube or shaft." This necessity of covering ventilating tubes or chimney tops to protect them from rain, does not exist in the case of soil pipes; these may only want protection against malicious introduction of stones or similar articles. A galvanized iron, copper or brass wire basket set into the mouth of the soil pipe will answer this purpose.

There is no doubt that open-mouthed pipes have a better upward ventilation than pipes covered with cowls, if the wind blows horizontally or nearly so. Wolpert in his "Treatise on Ventilation and Heating" states the average useful effect in per cents. of the velocity of the wind, as derived from a number of experiments, to be:

68.6 per cent. for open-mouthed tubes,
51.9 per cent. for pipes capped with Wolpert's new cowl,
35.8 per cent. for pipes capped with Wolpert's old cowl,

for a horizontal direction of the wind. In other words, the upward suction in a tube without any cowl is in the average equivalent to over $\frac{2}{3}$ of the force of the wind, blowing over it in a horizontal direction. For pipes capped with Wolpert's new cowl it is only a little more than $\frac{1}{2}$ of the wind force, and for the old cowl it is $\frac{1}{3}$ of it. As an average for other directions of the wind Wolpert finds the upward draft in pipes covered with his new and old cowls to be 51.5 per cent. and 34.5 per cent., respectively, of the wind force.*

The result of an elaborate series of about 100 experiments upon ventilating cowls, made on seven different days, at different times of the day, and under different conditions of wind and temperature, by Messrs. W. Eassie, Rogers Field and Douglas Galton, was as follows: "After comparing the cowls very carefully with each other, and all of them with a plain open pipe as the simplest, and in fact only available standard, the sub-committee find that none of the exhaust cowls cause a more rapid current of air than prevails in an open pipe under similar conditions, but without any cowl fitted on it. The only use of the cowls, therefore, appears to be to exclude rain from the ventilating pipes; and as this can be done equally, if not more efficiently, in other and similar ways, without diminishing the rapidity of the current in the open pipe, the sub-committee are unable to recommend the grant of the medal of the Sanitary Institute of Great Britain to any of the exhaust cowls submitted to them for trial."

FRESH AIR INLET.

The *second* requirement calls for a fresh air inlet or fresh air pipe. This is no less

*The current of air in these experiments was created by a powerful fan, the velocity of the current varying from 8 to 31 meters per second (from 17.9 to 69.3 miles per hour), equivalent to high winds and hurricanes respectively. The diameters of the cowls tested varied from 0.787 to 3.937 inches. It is to be regretted that the author did not extend his experiments so as to include much smaller velocities of current. It is very likely that for the latter the percentage of useful effect of cowls would be much smaller.

important than the extension of the soil pipes through the roof. In order to effect a constant movement and change of air in the pipes, two openings are required, an outlet and an inlet. The extension of the soil pipe through the roof provides only an escape for the foul air generated in the soil pipes and waste pipes through the decomposition of foul organic matter, clinging to the interior of pipes and lodging in traps under water closets and fixtures. But in order to oxidize and thus render harmless this matter undergoing putrefaction within the pipes, a constant introduction of fresh air from the outside atmosphere is necessary. As the soil pipe is warmer in winter time (being in the constantly heated house) than the fresh air pipe, located outside of it, an almost continuous upward current in the soil pipe results. In summer time this current is only seldom reversed; for as a general rule, the top of soil pipe is heated by the sun more than the fresh air pipe near the ground.

There is a second and almost equally important reason for providing a fresh air inlet, wherever the third requirement, the trapping of the drain, has been complied with. If a water closet is used or a pail emptied into a slop sink, the water discharged into the soil pipe acts like a piston; although it is not likely to fill a 4-inch pipe, it certainly carries the air on its course downward with it by friction. Thus the descending water drives air before it and out through the fresh air pipe; if this had not been provided, it would very likely force the nearest traps under fixtures, and send a puff of sewer gas into the living rooms. This reversed action of the fresh air inlet does not occur sufficiently often to warrant the apprehension of any danger in the location of the inlet. Of course, it should not be too near under windows of living rooms or dormitories, nor should it be placed too near the front steps of a city house. A little judgment should be exercised in locating the fresh air inlet. In cities, having between the house and the street a wide parking, it is best to build in this a small manhole, at the bottom of which the trap and opening for fresh air are located. The top of manhole should then be closed with a cover, having numerous openings so as to permit the outer air to enter the drain freely, and also to prevent as much as possible obstructions by snow or ice in winter time. For this reason it cannot be recommended to open the fresh air pipe into a gully in the sidewalk, or in the floor of an area. Equally objectionable is the location of the fresh air pipe in a coal slide. It seems best to carry the fresh air pipe some distance away from the house, and this is always practicable in the case of country houses, where the fresh air pipe should preferably be hidden from view by shrubbery.

If the main trap is located inside the foundation walls, the fresh air pipe should enter the drain just above the trap by a T or Y branch. Only in rare does cases it become necessary to carry the fresh air pipe vertically upward through the roof. This plan would neither be very efficient, as the difference in temperature of inlet and outlet pipe would be small, nor very economical.

As regards size of the fresh air pipe, I would say that nothing short of the diameter of the iron drain would answer; as this is generally 4 inches in diameter, a 4-inch opening for fresh air pipe is required. This opening should be protected against obstructions by a wire basket similar to that used for the upper part of soil or waste pipes.

TRAP ON MAIN DRAIN.

Our *third* requirement calls for a trap on the main drain between the sewer, cesspool or flush tank, and the fresh air pipe. A *trap* is practically a suitable bend or dip in the drain, which retains a sufficient quantity of water to prevent the passage of sewer gas.

The opinions of experts as to the advisability of trapping the main drain are divided, some considering the trap necessary, while others claim it should be omitted.

The objections urged against the use of traps are as follows:

1. They impede the ventilation of the public sewers.
2. They form an obstruction to the flow of the sewage in the house drain, and are, therefore, the cause of accumulations of foul matter in the drain, which by its decomposition will generate noxious gases; also
3. Foul matters will lodge in the trap.

While the first objection does not strictly belong to the subject of this paper I will say that it is accepted by most

authorities that house drains and soil pipes should not be used as ventilators for the street sewers. In exceptional cases —such as, for instance, where an entirely new sewerage system is built, designed and constructed according to uniform plans, and where not only the construction of sewers, but also the house plumbing is under constant supervision of the engineer and designer of the system*— the trap (and consequently the special fresh air pipe) may, perhaps, be left out. But I believe that a proper ventilation of sewers can be effectually carried out without ventilating through the houses.†

In regard to the second and third objections, I would say that obstructions do not frequently occur if the drain is carefully laid, with sufficient and continuous fall to insure a cleansing velocity of the flow. If such an inclination cannot be given to the drain, proper flushing appliances should be used, and these will by daily or more frequent washings, insure the removal of all matters liable to lodge in the trap. Another most necessary precaution to prevent accumulations in the trap, where the fall is very slight, may be found in the use of a proper grease trap, about which I shall speak hereafter.

No amount of care in laying the drain will prevent its obstruction through carelessly introduced articles; these will mostly lodge in the trap. A cleaning hole should therefore be provided with the trap, and is rarely omitted in good work, or else a Y branch, closed with a trap screw, should be inserted just a little above the trap.

In Vol. III. of the "Sanitary Engineer" will be found a discussion of the advisability of trapping the main drain. My own opinion, as stated in a communication to that journal, is as follows:

"If we could have *ideal* sewers, house drains and soil pipes, it might, perhaps, be possible to dispense with such a trap altogether. But since all sewers may have temporary stoppages from some cause, since house drains may settle or leak, and joints of soil pipes crack, thus allowing sewage matter to undergo putrefaction and enter the interior of houses, I would in all cases advise the use of a safeguard, consisting in a disconnecting trap and a *well ventilated soil pipe.* This latter arrangement is a *conditio sine qua non*, and rather than have a trap *without* ventilation I would advise to have none at all. . . . . I would always condemn as unsafe a system of house drainage in which the public sewers are ventilated through the houses. . . . . The work of ventilating public sewers should, in my opinion, be done by the same public authorities who devise the sewer system, and not by the householders."

Leaving aside, however, the case of a house drain connecting with a public sewer, it seems quite evident that, in the case of a house discharging its sewage into a cesspool, an effective barrier should be imposed to the gases constantly generated in that receiver of all foulness from the household; and equally so in the case of a flush tank which temporarily holds a large amount of fæcal and other refuse matter, which sometimes undergoes decomposition.

The principle of disconnecting each house from the street sewer was first advocated in England, and its importance becomes most apparent in the case of an epidemic, as by the use of a trap each house will be isolated, while if all houses have an open connection with a sewer, this and the house drains may become the channels for spreading the disease from one house to another. It has been said by those not in favor of such disconnection, that the air of the house drain, the soil pipe and the branch wastes is much worse than that of most city sewers, and that consequently no harm could be done by allowing the sewer to breathe through the pipes in the house. Such statement may be true in regard to the sewers of some cities; in others, sewers, especially if built long ago, are extremely foul. But it seems to me that just where the air of drains and pipes is foul, it needs a strong dilution and purification by abundant *fresh air*, which an opening to the outside atmosphere can furnish, but never a direct connection with a sewer.

An open connection of the house drain with a sewer or cesspool is necessarily based upon the condition that every joint in the house is perfectly tight, and every

* For instance, at Memphis, Tenn., and at Hamburg, Dantzic, Frankfort-on-Main, Berlin, Breslau, and other places in Germany.

† See Mr. Edward S. Philbrick's articles on "Ventilation of Sewers," in the Sanitary Engineer, Vol. I. See also Sanitary Engineer, Vol. V., Number 12, page 246.

trap perfectly trustworthy. As plumbing is done in most houses these conditions are only seldom fulfilled. But even where in new work such a standard of design and workmanship has been reached, the work may not remain so forever. It is, therefore, advisable to use a trap on the main drain as a safeguard, but in addition to this to insist upon occasional inspections. These become a necessity in the case of large buildings, such as hotels, schools, large factories, jails and almshouses.

Incidentally, it should be mentioned that a trap on the drain performs a most useful office during repairs or alterations of the plumbing work in keeping from the interior of the building the gases from the sewer.

Much, of course, depends upon a proper kind of trap for such disconnection. The old so-called "cess pool trap" is, next to the pan closet and the D-trap, the worst device ever proposed in connection with house drainage. As usually constructed it is of very large size, with square corners, and soon accumulates filth, becoming in a short time in reality a *cesspool*.

The common running trap, which is manufactured in earthenware as well as in iron is the simplest and at the same time the best of all forms. It should preferably have a vertical drop of a few inches from the drain to the water line in the trap in order to expel any solids that would tend to lodge in it. The running trap is often provided with a cleaning and inspection hole at the house side of the water seal, which serves as a fresh air inlet, when the trap is placed in a manhole outside of the house. In other instances a rain leader is inserted into the opening of the trap, which thus receives abundant flushing at each rain fall. The running trap is sometimes located on the line of the iron drain, just inside of the foundation wall, so as to be at all times easily accessible. A trap in iron, with a cleaning hole and a cover is then used. Care should be taken to close the cover perfectly air-tight.

In all cases the trap should be so located as not to be liable to freeze in cold climates or exposed localities.

In England various "disconnecting traps" have been used, such as Molesworth's trap, Prof. Reynolds' and Dr. Buchanan's disconnectors, Hellyer's Triple-Dip Trap, Pott's Edinburgh "air-chambered sewer trap," Stiff's "interceptor" sewer trap, Weaver's disconnecting trap, Mansergh's, Buchan's, Banner's, Stidder's, Bavin's traps, "Eureka" sewer air trap, and many others. All of these may have certain merits, but nothing could be better nor cheaper than the common running trap with fresh air pipe used almost exclusively in American plumbing.

For those exceptional localities where undue pressure in the sewer, from wind blowing into the outlet of the sewer, or from sudden changes of temperature (when exhaust steam is allowed to enter a sewer), or from heavy accumulations of surface waters gorging the sewer, or from the action of the tide in tide-locked sewers, frequently forces the seal of the trap, two running traps with a proper vent pipe between them have been recommended. I have myself, for some time, advocated such an arrangement, which, after further experience, I think complicated and unnecessary. It would require either a pipe extended through the roof, between the two traps, or else an open shaft (a manhole) between them, and besides this, in every case, a fresh-air pipe entering the drain above the upper trap.

TRAPPING OF FIXTURES.

The *fourth* essential, as stated above, calls for a suitable trap, placed as near as possible under every fixture.

As regards this point I cannot agree with the views of Prof. Osborne Reynolds of Owens College, Manchester. In his otherwise excellent little book, "Sewer Gas and how to keep it out of Houses," after explaining the necessity of a disconnecting trap on the main drain, and giving particulars about its construction, he continues: "There will then be no need to have traps within the house."

Traps under fixtures become a necessity, as much of the so-called "sewer gas" is actually generated in the drain and soil pipes of the house. Even the waste from a wash bowl becomes coated in time with a soapy slime, emitting bad odors. The trap on the main drain would offer no protection against the foul gases derived from organic matter decomposing within the pipes. We thus

see that, while some advocate the trap on main drain, but no traps under fixtures, others leave out the main trap, but trap the outlets of all fixtures. In my opinion, both the trap on main drain and those under fixtures are necessary.

Traps should be located as close as possible to fixtures, in order to reduce the length of waste pipe on the house side of the trap, which is liable to become foul with long use. Probably the best material for traps is lead, as this permits of making a good joint with the lead waste pipes. As Mr. Hellyer has truly pointed out, the junction of the trap with the waste pipe is of far more importance than its junction with the fitting, because the former is on the sewer side of the trap, and, unless properly made, would afford a passage for gases from the waste pipe system into the rooms.

Whatever kind of trap may be used under fittings (and there is an endless number of such patented devices), it is of the greatest importance that the trap should be *self-cleansing;* for this reason traps with square corners or large spaces, liable to accumulate dirty matter, are objectionable. Much depends on a proper size of traps for waste pipes: the smaller the trap the better will it be washed clean. As a good rule I would recommend to choose a trap a quarter or half an inch smaller than the diameter of the waste pipe, to which it is attached. The flushing stream is thus concentrated, and its scouring power increased within the trap, while on the other hand a trap an inch larger than the waste pipe is sure to fill up in time with sediment.

The following will serve as a guide:

Traps under water closets with 4 in. soil pipe should be 3½ in. to 4 in. diameter.

Traps under wash basins with 1¼ in. to 1½ in. waste pipe should be 1 in. to 1¼ in. diameter.

Traps under bath and foot tubs with 1½ in. waste pipe should 1¾ in. diameter.

Traps under laundry tubs with 1½ in. to 2 in. waste pipe should be 1¾ in. to 1¾ in. diameter.

Traps under sinks with 1½ in. to 2 in. waste pipe should be 1¾ in. to 1¾ in. diameter.

Traps under slop sinks with 2 in. to 3 in. waste pipes should be 1¾ in. to 2 in. diameter.

As regards the proper dip of traps I would say that traps under those fittings which receive solids (water closets) should not have a greater dip than 1½ to 2 inches, because otherwise the solids are not readily removed, and lodge in the trap. For traps of minor wastes a larger dip or "water seal" is advantageous, as affording a protection against loss of seal through evaporation, siphonage or back pressure.

Traps may be classified according to the means used for the exclusion of gases into:

1. *Water-seal traps.*
2. *Mechanical traps.*

The characteristic of all water-seal traps is that they have in their lowest part a bulk of water divided by a dip in the pipe, so as to stand on the house side as well as on the sewer side one or several inches higher than the lowest point of the dip, thus making a seal which, under ordinary circumstances, prevents the passage of gases.

The traps of the second class have, in addition to the water-seal, a mechanical contrivance such as floats, balls, valves, flaps, &c., to exclude sewer gas.

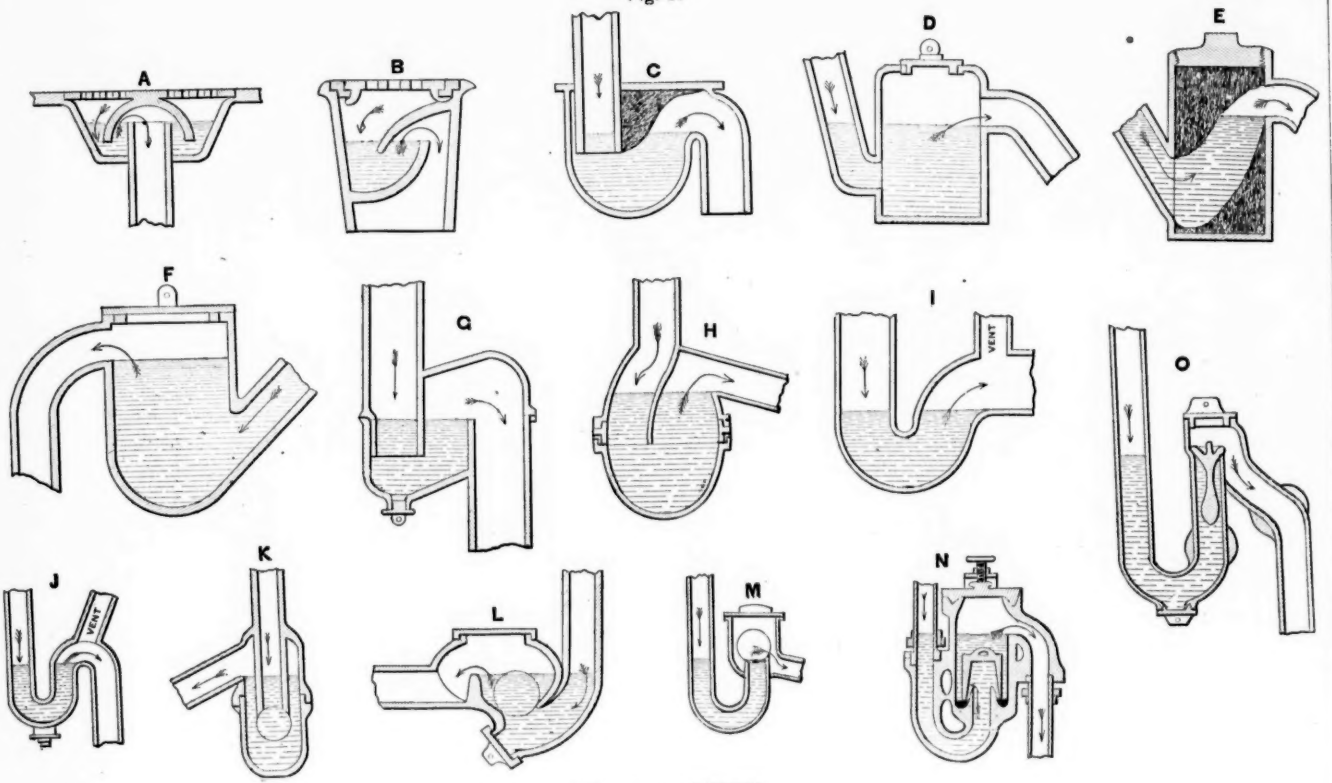
Of water-seal traps I mention the bell trap, Antill's trap, the old fashioned D-trap, the bottle or round trap, Adee's trap, the Climax trap, the common S-trap, P-trap and three quarter S-trap. There is an endless variety of mechanical traps, amongst which I mention Bower's trap, Cudell's trap, Garland's trap, Buchan's trap, Waring's check valve, Nicholson's mercury seal trap, and others (see Fig. 2.)

The bell trap A is objectionable on account of insufficient water seal and improper shape. It is frequently found at the outlet of sinks and yard gullies, and being in its upper part a movable strainer, it is often lifted by servants or thoughtless persons, and the gases from the drain pipe thus enter the house freely.

Antill's trap B avoids this defect, having a *fixed* strainer, but is objectionable on account of shape and small water-seal.

The D-trap C and the bottle trap D constitute small cesspools; they violate the principle that a trap ought to be self-cleansing. The D-trap accumulates dirt and grease in the upper corner, which receives no scouring from the water passing through the trap; and the bottle trap very often chokes up as shown at E. A round trap of improved shape is shown at F, which may keep cleaner on account of its round bottom.

Fig. 2.



TRAPS FOR FIXTURES.

Adee's trap G is little better in this respect, though it has this to recommend it that it is not so easily siphoned, having a large air space above the water, and a large body of water in the trap. This is also true of the round trap, when new and clean; when choked with grease as shown at E, it is as much liable to siphonage as the S-trap.

The Climax trap, H, has a large dip and a round cup at its bottom, which is removable for cleaning purposes. Its resistance to siphonage is not greater than that of any of the other traps, or that of the common S-trap with same depth of water seal.

The P-trap I, and S-trap J, are shaped so as to be perfectly self-cleansing when adapted in size to their waste pipes. They are of uniform diameter throughout, have no nooks or corners to accumulate dirt. The old hand-made S-traps with seams have been superseded by lead traps cast in a mould such as the Du Bois traps. As regards cleanliness these traps are undoubtedly superior to all other traps of which I have knowledge. They cannot, however, be relied upon to exclude sewer gas, as their water-seal is frequently destroyed either by siphonage or by evaporation. They are shown in Fig. 2, with a vent pipe attached at the highest bend of the trap on the sewer side of the seal. The object of this vent pipe is to prevent siphonage, as will be explained hereafter.

Bower's trap is shown at K. This trap has a water-chamber into which the pipe from fitting enters at the center, and an outlet pipe on one side. The mouth of the inlet pipe is sealed by the water in the chamber, but in addition to this a floating ball of india-rubber in the water chamber is held tightly against the mouth of the inlet pipe, forming a seal, which, however, depends on the quantity of water in the chamber. The water, in passing through this trap, removes the ball from its seat and rotates the same, thus keeping it clean and free from matters adhering to it. An additional advantage of this trap lies in the ball, which, being compressible, allows the water in the chamber to freeze without danger of the bursting of the cup. Unless the soil pipe is extended full size through the roof this trap may have its water lowered by siphonage so much that the ball will drop from the mouth of the inlet pipe, but with proper ventilation of soil and waste pipes it forms an efficient trap for wash bowls, tubs and sinks, although it is not as self-cleansing as the common S-trap.

Waring's check-valve is shown at O. This valve forms a seal by its weight, and the seal is dependent upon the accuracy of the turned seat. Hair and particles of other matters may adhere to it and prevent a tight shutting of the valve.

Cudell's trap L and Buchan's trap M are constructed much upon the same principle, but have a heavy metallic ball instead of a conical-shaped valve. This ball may keep cleaner by being revolved, but in this case, as above, the tightness of the seal will depend upon the accuracy of turning the seat.

Nicholson's mercury seal trap N has an inverted porcelain cup inside of its cylinder, the edge of which rests on mercury, forming a tight seal. The cup is lifted, at each discharge, by the force of the water entering at bottom of cylinder; after all water has passed from the basin the cup falls back in its place. This trap is generally made of earthenware with brass couplings; it is therefore a more expensive trap, but the mercury seal very efficiently prevents the entrance of sewer air, even if the water in the cylinder should be removed by siphonage or evaporation.

VENTING OF TRAPS.

The *fifth* requirement asks for a proper vent pipe for such traps under fixtures as are liable to be siphoned. This siphonage constitutes in many cases a danger, but especially so with S-traps. Traps may be siphoned under the following conditions:

1. Traps with an easy bend, on a rather steep line of waste pipe, and with small depth of seal, are liable to empty themselves by the momentum of the water rushing from the fitting through them. The air in the upper bend of the trap is expelled and replaced by water, which causes the trap to act as a siphon. When the fitting has discharged all its water, and air breaks the siphon, the water in its inner limb will mostly drop back into the trap, but in case of a small dip it would be insufficient to seal the trap.

Unless a slow after-flush takes place the trap remains unsealed.

2. Traps under fixtures may be siphoned by a flow of water coming from another fitting on the same branch waste pipe.

3. Traps may be siphoned by a discharge—from a water closet, a tub, or from a pail of water from a slop sink—into the main soil pipe, to which the branch waste of the trap is connected.

To guard against the *first* danger the dip or water seal of the trap should be as great as possible; but, even then a special vent pipe will often be necessary, attached to the highest part of the bend in the trap on the sewer side of the water-seal, or else a mechanical trap should be used.

To guard against the *second* danger the trap of each fixture should be vented; wherever possible, each fixture should discharge independently into the soil pipe, thus reducing the danger from siphonage to cases 1 and 3.

The *third* danger from siphonage by a discharge into the main soil pipe, either above or below the point where the waste from the trap enters it, will in some cases be sufficiently prevented by the complete and thorough ventilation of the soil pipe. In many cases, however, the venting of the trap becomes necessary.

Where a number of water closets discharge into the same inclined branch of a soil pipe the air-vent to the water closet trap becomes necessary, especially so with water closets, discharging quickly a large body of water, such as the various patterns of the plunger closets (Zane, Demarest, Jennings) and some of the "wash-out" closets.

Where slop hoppers are trapped by an S trap, this must be properly guarded against siphonage, as the trap is very likely to lose its seal from the momentum of the water rushing through it each time a pail of slops is quickly emptied into the sink.

The material most suitable for air pipes is lead, as such pipes are easily joined to lead traps. Sometimes wrought-iron tubing is used, and, since the vent pipe is not so much intended for carrying off foul gases [which office is performed by the vertical extension of all waste pipes through the roof] as to afford a passage to air in order to break the suction, they may be safely used. Care should be taken to lay these pipes with a slight inclination, in order to prevent accumulation of water from condensation in the pipes. Vent pipes for fixtures on different floors may be joined, if convenient, and may enter the soil pipe above the highest fixture. But it is preferable to run them to a main vent pipe of lead, or better, cast iron, which goes through the roof independently. Where this passes through the roof it must be enlarged to 4 inches diameter, as it might otherwise be obstructed by ice in winter time. It should not be covered at the top with any kind of ventilator. The size of the vent pipe should never be less than that of the trap, except for water closet traps, where it should be 2 inches in diameter, but in the case of two or more water closets it should be 3 inches and sometimes even larger from the point where the various vent pipes join.

It is often not only costly but also inconvenient to run vent pipes to the roof.

There is also some danger that the vent pipes for traps under tubs, sinks and bowls may stop up with soapsuds or grease, in which case they would cease to act properly. The continuous current of air in the vent pipe, in passing over the water in the trap, will tend to increase its evaporation. Finally it becomes necessary in the case of high buildings, largely to increase the diameter of vent pipe in order to make up for the loss through friction necessarily occurring with long air pipes. Therefore, while I consider vent pipes for traps a necessary evil in many cases, I am inclined, in other cases, to prefer a good mechanical trap, which cannot be siphoned, provided the soil and waste pipe system has ample ventilation. Such mechanical trap may be used under sinks, tubs and bowls; but for water closets and slop hoppers (if without a strainer) the simple lead water seal trap with vent attached is the only safe device.

EVAPORATION OF WATER IN TRAPS.

Nothing short of continuous use of the fixtures will prevent evaporation of the water in traps. A large dip is recommended for traps on waste pipes to guard against a rapid loss of the seal. When a house will be left unoccupied for a long time, but especially during the hot sum-

mer months special precautions should be taken to prevent sewer gas from entering the rooms and saturating carpets, wall-paper and furniture. Replacing the water in traps with oil or glycerine may be recommended, or else the use of common rock salt which attracts sufficient moisture from the atmosphere to make up for the loss by evaporation.

ABSORPTION OF GASES BY THE WATER IN TRAPS.

It is well known that water has the property of absorbing gases, and it was believed that the water in traps would readily absorb sewer air from the soil pipe and give it off at the house side of the trap by evaporation. It has also been asserted that microscopic organisms (germs of disease) floating in gases of decay would pass through the dip of the water-seal and enter the house through the fixtures, and that consequently the water-seal of traps offered no security against the invasion of sewer gas. Dr. Fergus, of Glasgow, Scotland, was the first to call attention to this matter, and made an extensive series of experiments in 1873–74, which led him to condemn as unsafe the system of water carriage in general, and the trapping of fixtures. The views of sanitarians, based upon Dr. Fergus' experiments, have been much modified by recent experiments of Dr. Carmichael, of Glasgow, by researches of Dr. Frankland in London, Wernich and Naegeli in Germany, Prof. Rafael Pumpelly and Prof. Smyth in Newport, R. I., and others.

Dr. Fergus' experiments were made with gases in a concentrated condition, and as such are quite as reliable as the more recent experiments. But the latter more closely resemble actual cases, being made by experimenting directly with soil pipe gases. Referring to what has been said about sewer gas, it will be seen that ammonia, sulphuretted hydrogen and other gases of decay are present in drains and soil pipes only in minute quantities. Dr. Carmichael found that the amount of these gases passing through a water-seal trap was so extremely small that no danger could be apprehended. With a thoroughly ventilated system of soil and waste pipes this peril may be taken as insignificant.

Another set of experiments by Dr. Carmichael, made to determine the passage of germs through water, seems to indicate that germs, even if contained in the water of traps, are not liberated from it, as was hitherto supposed, unless the water is violently agitated. Frankland in England, Naegeli in Germany and Prof. Pumpelly in Newport, R. I., arrived at the same conclusion, after careful investigations and experiments.

Dr. Carmichael sums up his conclusions by saying: "Water traps are, therefore, for the purpose for which they are employed, that is, for the exclusion from houses of injurious substances contained in the soil pipe, perfectly trustworthy. They exclude the soil pipe atmosphere to such an extent that what escapes through the water is so little in amount, and so purified by filtration, as to be perfectly harmless; and they exclude entirely all germs and particles, including, without doubt, the specific germs or contagia of disease. . . . ." Further scientific researches will undoubtedly throw more light on this yet little investigated subject.

TRAPS FORCED BY BACK PRESSURE.

It has already been explained how traps under fixtures may be forced by back pressure. This cannot, however, occur with traps under fixtures, if all soil and waste pipes are properly extended through the roof, and provided with a fresh air opening at their foot.

BRANCH WASTES FROM FIXTURES.

Fixtures are connected to the soil and waste pipe system by branch wastes carried under the floors. The material used almost exclusively for such branch wastes is lead, and the sizes adapted to different fixtures have already been stated. The connection is very simple in the case of a single fixture, such as a kitchen sink, or a lavatory. The problem becomes more intricate in the case of a set of fixtures, such as are generally located in a bath or dressing room. A bath room of the better class of city houses contains a water closet, a bath tub, and a lavatory, sometimes also a hip-bath or bidet. It is desirable that each of these fixtures should have a separate connection to the soil pipe. Such is seldom possible, except when the soil pipe is located in a special shaft, or where it is possible to conceal the pipe and Y branches by a

"false ceiling," as the height of timbers does not generally allow of the placing of more than one Y branch.

A very common, but most defective manner of overcoming the difficulty is by emptying the wastes of bath tub and bowl into the water closet trap below its water line, supposing the water closet to be of such type as requires a lead trap below the floor. As the waste pipes have only a slight fall to the trap, the water of the latter, which frequently holds excremental matter, will stand for a long distance back in the waste pipe and keep it continually foul; the free flow from the bath and bowl is much retarded, the waste being air-bound between the water closet trap and the traps of bowl and bath. Matters are even worse, when the water closet trap is meant to serve also as trap for the bowl and bath, these having no traps placed under them. The foul water standing back in the waste pipes will then readily evaporate into the dressing room, and fill it with noxious odors. Moreover, it frequently happens that this trap becomes displaced by tipping over, or that the waste pipe attached to the trap sags, so as to render the water seal, which is rarely over an inch in depth, ineffective. It will be readily understood how, under such circumstances, the foul gases of the soil pipe—especially if this be unventilated, as is so often found in examining old houses—gain an easy access into our rooms. Should the main drain have an untrapped connection to a sewer or cesspool, the gases from these would ascend and permeate the whole building. Such instances of faulty work are by no means rare, and are causes of much preventible headache and sickness.

To run such wastes into the water closet trap above its water line is equally wrong.

Where the water closet is some distance away from the soil pipe, it is possible to insert between its trap and the junction with the soil pipe, on the horizontal part of the soil pipe, two 4″ × 2″ Y branches, or else one double Y branch for bath and bowl wastes. Where the water closet is quite near the soil pipe, and the connecting pipe between them is of lead, the wastes from bowl and bath may join the latter beyond the trap. Wherever there is room enough, a 4″ × 2″ double Y branch may be inserted vertically below the water closet branch on the soil pipe, or else one 4″ × 2″ Y for bowl above the water closet branch, and a 4″ × 2″ Y below it for the bath waste. It seems desirable that the iron works should manufacture a combined Y branch, having a 4-inch opening for the water closet waste, and one or two 1½ to 2 inch openings for the smaller wastes.

Long lengths of waste pipes under floors are objectionable; to avoid them it is sometimes better to provide a special stack of 1½ to 2 inch vertical iron waste pipe near lavatories or baths, where these are remote from the main soil pipe.

It is customary to provide bath tubs, wash bowls, and pantry sinks with an overflow pipe, in order to prevent flooding of floors, if the outlet of any of these fixtures should be closed by a plug, and the water carelessly left running. These overflow pipes should enter the waste between the fixture and its trap, or else they should enter the trap below the water line, so that the trap serves for both waste and overflow. Overflow pipes do not receive a thorough flushing, and are liable to become foul with soapsuds, emitting unpleasant odors. For baths, fortunately, the overflow pipe can be safely dispensed with by using the standing overflow, for bowls those with "patent overflow," *i.e.*, a concealed channel in the earthenware bowl, have the length of overflow reduced to a minimum.

A set of laundry trays is generally trapped by only one trap, thus leaving a long length of waste pipe in connection with the air of the room. I believe, however, that such wastes, properly restricted in size, and laid with sufficient inclination, can be kept well flushed and clean, and therefore unobjectionable.

In the case of a set of water closets or urinals I consider it imperative to have a separate trap under each fixture.

It is of the utmost importance that the connection between water closet and soil pipe should be *absolutely tight*. The different types of water closets are provided at their outlets either with a lead trap under the floor, or else they have a trap of iron or earthenware, as the case may be, above the floor, or they are so-called "trapless" closets, in which case the only water-seal against gases is formed by the water held in the bowl (either by a valve, pan or plunger, or by a special shape of the bowl). For water

closets having a lead trap under the floor a brass ferrule is connected by a wiped joint to the end of the trap, and the ferrule is inserted into the hub of the iron soil pipe, and caulked tightly. The house end of the lead trap is flanged out, and the earthenware or iron horn of closet inserted into it, resting with its horizontal flange upon a ring of soft india-rubber, or of oakum, saturated with red lead. Wood screws, drawn through the horizontal flange into the floor, tighten the connection.

In the case of trapless closets and such with trap above the floor, the outlet is generally connected by a lead thimble to the soil pipe in the same manner as just described for lead traps.

Such a connection is in neither case a perfect one. But in the case of closets with trap under the floor, this connection is on the house side of the trap, and the danger from leakage of sewer gas from the soil pipe is prevented by the water seal. With trapless closets (such as some pan closets, valve closets and plunger closets), with closets having trap above floor (short hopper, some plunger closets), and finally with all "washout" closets such a connection is dangerous, and a better joint than is used at present should be devised, such as, for instance, a connection by means of a brass ferrule between water closet outlet and iron soil pipe.

SAFE-WASTES.

In order to prevent the flooding of floors and ceilings, fixtures, such as wash bowls, bath tubs, water closets, etc., are mostly lined with a safe of sheet lead, provided with a waste pipe. In bad plumbing work these "drip pipes" are either joined into the nearest soil or waste pipe—often even without a trap—or else, in the case of water closet safes, are made to run into the water closet trap. Such drip pipes should not be connected at all to the drainage system. They should run vertically downward to the cellar, and open either over a sink, or terminate at the cellar ceiling. Should it be feared that the drip pipes might become the channels for leading the cellar air into the upper rooms, their mouths should be closed with paper, glued over them, or the pipes should have an upward bend, closed by a ball, which is prevented from dropping by wire bands.

RAIN LEADERS.

Rain-water pipes may be of galvanized wrought-iron, or of tin; when laid inside of a house they should be of cast iron and their joints treated in all respects as those of soil pipes. Before joining the house drain they should be trapped, if such junction is made beyond the main running trap of the drain, and the trap of the leaders should be sufficiently deep in the ground to prevent the water from freezing. If rain leaders join the drain inside of the house they should not have a special trap, unless their top opens near dormitory windows. Sometimes a leader delivers into the main trap of the drain, and thus helps to cleanse the trap.

Rain leaders should never be used as soil pipes nor should they be solely depended upon to ventilate the drain; and, on the other hand, soil pipes should never be used to carry rain water from the roof.

In making a sanitary examination of the Executive Mansion at Washington, under direction of Col. Geo. E. Waring, Jr., the writer had occasion to see an instance of the violation of this rule. The main soil pipe in the building was a 10-inch (!) cast iron pipe, which served the double purpose of receiving the discharge from three water closets, a urinal, a slop sink and some wash bowls and bath tubs, and also all the rain water from the large roof. At each rain-fall this large pipe received ample flushing, but in times of prolonged droughts its inner walls became thoroughly slimed and foul with excremental and other matter. In times of violent rain storms the water rushing down the 10-inch pipe and passing the branch wastes, very likely siphoned all water out of the traps, thus leaving the house unprotected against the foul gases of the soil pipe.

CISTERN OVERFLOW PIPES.

Both under-ground cisterns and cisterns in the attic of a house should be provided with an overflow. The usual custom has been to connect this overflow pipe to the drain, or, if inside a house, to the soil pipe. In consequence of this most pernicious practice the water was contaminated, and since water is known

to be a carrier of disease germs not less so than the air, sickness and deaths were traced to this faulty arrangement.

No overflow from a cistern for cooking, washing or drinking water should be connected to any part of the drainage system under any circumstances. Even if properly trapped the danger is not removed, as the water in this trap evaporates, and as an overflow seldom occurs, no water refills the trap, and drain air passes freely into the tank. This overflow should be made to run into the gutter of the roof, wherever this is practicable. In cold climates or in exposed places its outlet should be protected by a flap-valve. If, for some reason, the above course cannot be followed, the overflow should discharge over an open sink in the basement or cellar. If the cistern is located outside of the house, the overflow should be carried to some low point, where it should have an open outlet. Blow-offs for water-tanks should be treated similarly to the overflow-pipe.

REFRIGERATOR WASTES.

It is not safe to have a direct connection between a refrigerator waste and drain or soil pipes, for reasons given above for overflows of cisterns. Small refrigerators may waste into a pail to be removed and emptied periodically. Wastes from large refrigerators should empty over an open cup with a waste at its bottom, provided with a reliable mechanical trap and connected to the nearest soil pipe or drain.

DRAINAGE OF CELLARS.

It remains to discuss the proper method of removal of excessive moisture from the soil under and around a dwelling. Unless this is properly attended to, cellars of houses will be continually damp, the brick or stone walls will readily absorb the moisture by capillary attraction and an excess of watery vapor will fill the house. The well known researches of Dr. Bowditch of Massachusetts, and of Dr. Buchanan in England, have clearly established the relation of excessive soil moisture to certain diseases, notably *consumption*, bronchitis, pneumonia and other diseases of the lungs.

Dr. Parkes, in his admirable "Manual of Practical Hygiene" speaks about diseases connected with moisture and ground-water as follows: "Dampness of soil may presumably affect health in two ways—(1) by the effect of the water, *per se*, causing a cold soil, a misty air, and a tendency in persons living on such a soil to catarrh and rheumatism; and (2) by aiding the evolution of organic emanations. The decomposition which goes on in the soil is owing to four factors, viz.: presence of decomposable organic matters (animal or vegetable), heat, air and moisture. These emanations are at present known only by their effects; they may be mere chemical agencies, but more probably they are low forms of life which grow and propagate in these conditions. At any rate, moisture appears to be an essential element in their production. The ground-water is presumed to affect health by rendering the soil above it moist, either by evaporation or capillary attraction, or by alternate wettings and dryings. A moist soil is cold, and is generally believed to predispose to rheumatism, catarrh and neuralgia. It is a matter of general experience that most persons feel healthier on a dry soil."

In order to keep the level of the subsoil water below a certain depth artificial channels should be provided, laid at that depth and sloping towards some proper outlet which will remove all surplus water. These channels, which carry off only clean water, are also called *drains* (this being the original meaning of the word).

Under the foundation walls of the house trenches dug for this purpose should be filled with loose or broken stones. Drains (common tiles) should be placed two or three feet below and under the cellar floor, with open joints, care being taken to prevent any intrusion of earth at the joints, by wrapping tarred paper or strips of cotton around them. The drain can then be covered up and buried. The size of the tile drains will depend on the character of the soil. As a general rule 1½-inch tiles are quite sufficient, except in the case of a spring in the cellar, when it may be necessary to use pipes of 2 inches and sometimes even larger sizes.

The only difficulty, from a sanitary point of view, consists in finding a proper outlet. If the house is a country residence with ample ground around it, and especially if the land is not level, but

slopes to some distant valley or creek, it is very easy to continue the main cellar drain with a sufficient pitch to some gutter or open ditch, into which it may discharge.

The case becomes difficult with city houses, on narrow lots, with no other outlet available but the sewer under the street. A direct connection between the cellar drain and the sewer is forbidden for well-known reasons, and even the interposition of a water-seal trap may not be regarded as a sufficient safeguard, for during periods of droughts the water evaporates, allowing the gases from the sewer to pollute the ground under the house.

The drain should run into a mason's trap with *deep* water-seal, and filled with coarse sand or fine gravel, and before joining the sewer the drain should be trapped by a running trap, into which, if practicable, a leader should discharge. Another arrangement is to trap the cellar drain, and to provide an outlet for gases which may force the trap, by a vertical pipe, on the house side of the trap, and opening on the surface of the ground. This is sometimes done when the sewer is in an alley at the rear of the house, and an open yard gully may be connected to the vertical vent pipe to supply the running trap with water.

It is equally important to have a dry, impervious floor in the cellar, which can be secured by first laying a base of concrete, upon which a layer of about $\frac{1}{4}$ inch of asphaltum should be placed. This makes the floor practically impervious. It should then be properly finished with a layer of best Portland cement.

DAMPNESS OF WALLS.

In order to prevent dampness of walls, that part of the wall below the level of the ground should be constructed with particular care. Nothing will better prevent dampness in walls than a "damp course" of some impervious material. Asphaltum is probably best for this purpose, though layers of slate in concrete or damp proof tiles are very efficient. If at all practicable there should be a dry area all around the foundation walls in order to prevent any dampness in the walls originating from the earth surrounding it at the sides. If such an area cannot be provided a *double* wall with an air-space between inner and outer walls should be used.

SYSTEM OF HOUSE DRAINAGE.

Fig. 3 represents a section through a dwelling house, illustrating the essential elements of a system of house drainage.

A is the gravel trap, into which the subsoil drain B discharges, and which serves to prevent the gases from the sewer from entering the drain tiles and permeating the cellar. The drain B for cellar drainage should be of common 1¼-2 inch tile drains, laid with open joints, around which tarred paper or cotton rags may be wrapped to prevent any stoppage of the tiles from dirt falling in at the joints.

C is the house drain, which should consist of 4-inch vitrified pipe with well cemented joints to within 10 feet from the cellar wall. D is the running trap on the main drain to disconnect the house from the sewer. Into it the rain leader X discharges. E is a Y branch, closed with a brass trap screw, for cleaning purposes. F is a fresh air pipe, 4 inches in diameter, entering the house drain above the trap, and carried some distance away from the house, its mouth being hidden from view by shrubbery, and covered with a wire basket for protection against obstructions.

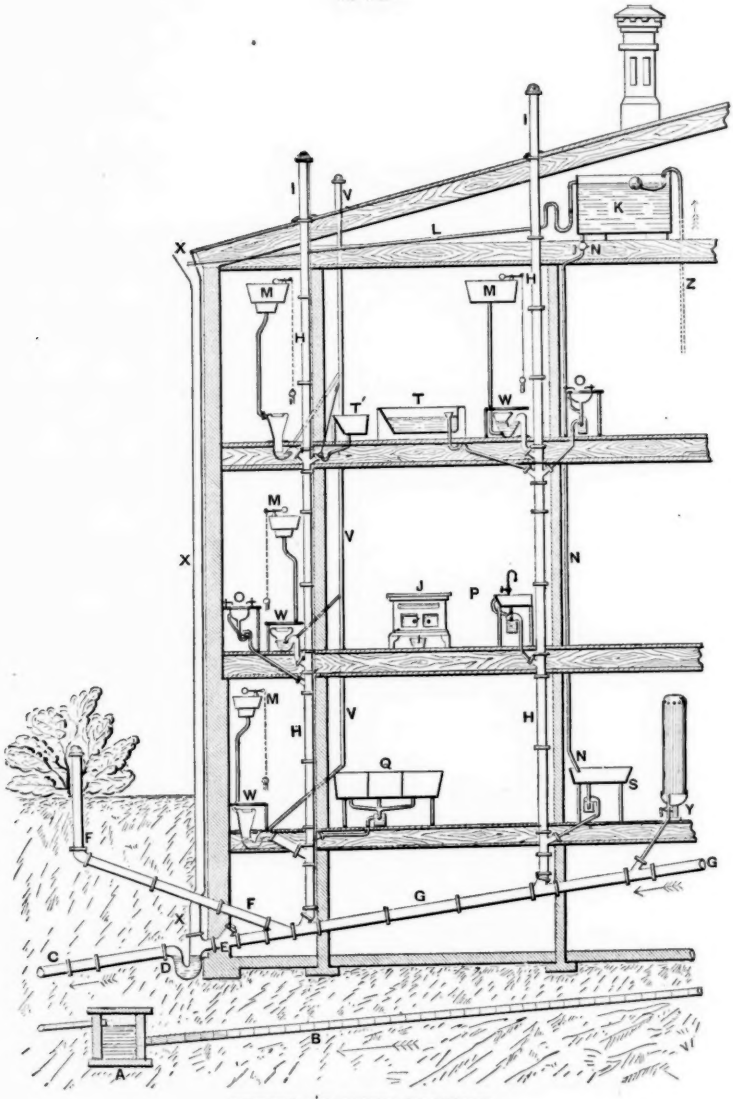
G is the 4-inch house drain, of heavy iron pipe, with well caulked lead joints, carried with sufficient fall along the cellar wall to the furthest point, where it receives either a soil pipe or a rain leader.

H H are the 4-inch iron soil pipes, which join the iron drain in cellar by Y branches and eighth bends. They are extended full size through the roof, and their outlets I I are protected by a strong wire basket.

J is a small refrigerator which wastes into a movable pail. K is the large tank in attic, which is supplied through a ball-cock from street pressure. Its overflow pipe L is shown trapped by an S-trap with deep seal, and emptying into the gutter of the roof. The blow-off N from tank runs down vertically and delivers over the kitchen sink.

M M are small cisterns for flushing the water closets and slop hopper *only*.

Fig. 3.



SYSTEM OF HOUSE DRAINAGE.

O O are earthenware wash bowls with 1¼-inch waste pipes and overflow pipes of lead, trapped by Cudell's or Bower's traps, and delivering into 4″ × 2″ Y branches of soil pipes.

P is a pantry sink, of heavy tinned and planished copper, with overflow and 1¼″ waste pipe of lead trapped by a Bower's trap and entering a Y branch of soil pipe.

Q are cement stone or ceramic wash tubs, with 1½″ waste pipe, and trapped by a Bower's trap.

R is an all-earthenware flushing-rim slop hopper, trapped by a vented S-trap, and flushed from a special cistern.

S is the kitchen sink, of galvanized or enamelled iron, or of earthenware, trapped by an $1\frac{1}{4}''$ Bower's trap with $1\frac{1}{2}''$ lead waste pipe.

T is a bath tub, of enamelled iron, or heavy planished copper or of porcelain. It is provided with a standing waste, and trapped by an $1\frac{1}{4}''$ Cudell running trap. T' is a small hip bath, of copper, provided with overflow and $1\frac{1}{2}''$ waste pipe, trapped by a vented S-trap.

V is a 2-inch air pipe to prevent the siphonage of traps. It is extended through roof, and enlarged to a 4-inch outlet, which should be left without any other covering than a wire basket. Into this air pipe enter the vent pipes from S-traps under slop hopper, water closet and hip bath.

W W W are water closets, the types shown being the long and short hopper and the washout closets. Each of these is provided with a special flushing cistern M M M.

X X is a rain leader delivering the water into the running trap of the house drain.

Y is the blow-off from the boiler, which wastes into a Y branch of the iron drain in cellar.

The system described and illustrated differs from the methods of house drainage as practiced in England in one essential point. There, it is the rule to keep soil pipes separate from waste pipes, to deliver to the former, in the words of Prof. Fleming Jenkin, "such foul matters as would certainly be tainted when contagious disease occurs in the house," in other words, the waste water from water closets, urinals, slop sinks and probably laundry tubs; a second system "receives all liquids, which may be called dirty, but not foul—the water from baths, kitchen sinks, and wash hand basins." It is, moreover, the rule in England to locate the soil pipe outside of the house walls, and to deliver the waste pipes over an open gully in the yard, from whence the wastes run into the house drain. Both arrangements are entirely impracticable in this country on account of the severity of the climate, and the separation of the two systems by discriminating between foul and dirty waste water leads to unnecessary complications. With well jointed, thoroughly ventilated soil pipes of iron, it seems quite permissible in American plumbing to run into them the wastes from any fixture in the house, if it be near the soil pipe, and where vertical stacks of waste pipes are run for bath tubs and wash basins, these waste pipes, if properly jointed, may with perfect safety deliver into the iron cellar drain, which receives the soil pipes of the house.

If all the given rules are carefully observed, the system of drainage of a dwelling will be as perfectly as possible in accordance with the present knowledge of sanitary science. Time and experience may find out hitherto unknown faults, but will also, it is believed, teach the proper remedy. *With pipes of proper material, properly joined, properly laid, and properly and sufficiently often flushed with air and water, the object of a system of house drainage seems to be attained, viz., the instant removal from the house of all liquid and semi-liquid waste matter, and the perfect oxidation and constant dilution of the air contained in the pipes.*

Says Mr. J. C. Bayles: "The conclusion I have reached is that when sewer gas finds its way into a house through the soil and waste pipes, the fault lies somewhere between the architect, the builder and the plumber. In any case, it is without excuse. I know that houses can be drained into sewers—without bringing sewer gas into them. The existence of foul sewers is in itself a perpetual danger to the public health, but there is no reason why we should bring that danger into our houses by providing channels through which the poisonous air of the sewer can find a means of ingress. I know of houses into which no sewer gas ever comes — unless, possibly, through the windows, borne in with the air of the street — and I have no hesitation in saying that, when the tenants of houses demand immunity from the dangers of unhealthful conditions, architects and builders will find a means of correcting the evils now complained of as practically irremediable. Sanitary reform in cities only waits until those to be benefited by it shall demand it."

RECORD AND PLAN OF DRAINAGE AND PLUMBING INSPECTION.

It cannot be too strongly recommended to every householder to keep for future reference, for cases of inspection or repairs and alterations, a complete plan of all the drain, soil and waste pipes in and outside of the house, a record of the depth of the drain, of the sizes and material of pipes, of the location of junctions, traps, fresh air pipes, access pipes or cleaning Y's, of all fixtures on every floor, etc.

Frequent inspections of the plumbing of buildings are by no means superfluous. They are very important in the case of public buildings, schools, hospitals, asylums, jails, hotels, but especially so, for such buildings as are occupied only a part of the year (summer residences, seaside hotels, mountain resorts, etc.). In some cities "sanitary associations" have been organized, such as at Newport, R. I., Lynn, Mass., Brooklyn, N. Y., and other places. The members of these associations can avail themselves of the services of an inspector of plumbing employed by the association, in order to assure themselves by frequent inspections of the sanitary condition of the plumbing in the house, of its outside drainage and water supply, its ventilation, etc.

In the case of new buildings the architect's plans should show the exact location of the proposed plumbing work in the house. The work should be done according to written specifications, carefully drawn up by the architect or a sanitary engineer, under whose immediate direction the plumber should work. It is a mistake — but, alas! how often is it made — to give the plumbing work of a new building out by contract. The slight amount saved in first expense is almost always followed by an increased outlay for repairing and altering defects, which appear only after the house is occupied. A prudent house owner will prefer to have his plumbing done by day labor, by honest, conscientious plumbers — and these are by no means rare, as the universal cry against them would seem to indicate — who care more about their reputation than about a few dollars earned through dishonest and reckless work.

PLUMBING REGULATIONS.

The cities of New York, Brooklyn and Washington lately have set an example worthy of imitation in other cities. The health authorities have issued excellent regulations for plumbing of buildings, and require the plans for plumbing to be submitted to them for approval and for filing. The plumbing, before being covered up, is examined by intelligent inspectors of the Board of Health. There may be at first some bad feeling about such a measure, but the good plumber will soon understand that the law passed is to his advantage; it will protect him against the "botchers" in the trade, and will help to re-establish his of late much abused good name.

These plumbing regulations will certainly tend to lessen the frequent complaint about bad plumbing in houses, and the consequent entrance of sewer gas. They will contribute much towards the lowering of a high death rate, and similar regulations may be adopted with advantage in all large cities.

THE RUSSIAN ARSENALS.—The production of the various Russian arsenals and gun factories during the year 1880 was as follows: The gun factory of Toula turned out 135,000 infantry rifles, and 15,000 cavalry carbines. That of Sestroretzk 120,000 rifles and 5000 Cossack carbines. The Tjer workshops supplied 130,000 rifles, 5000 Cossack carbines, and 125,000 gun-barrels. The private factory at Zlatwost furnished 15,833 swords and 25,000 gun-barrels, and actions were purchased from the Oboukhov Steel Works. The arsenal at St. Petersburg completed 150 short bronze 24-pounder guns, and supplied the breech-blocks for 435 steel guns, which were manufactured at the Oboukhov works; 50 6-in bronze mortars were constructed at Biransk. The different arsenals also delivered 270 iron field gun carriages and wheels, 648 iron limbers, with wheels and ammunition boxes, 378 ammunition wagons, 20 siege gun carriages, together with a large quantity of wheels and extra fittings; 2500 tons of powder were produced at the factories of Okhta, Chostka, and Kazan; 151 millions of cartridges, a large quantity of caps, &c., were completed at St. Petersburg; and the rocket factory at Nicolaiev turned out about 5000 rockets of various kinds.

VENTILATION OF SEWERS.

From "The Architect."

A REPORT has been prepared by Sir Joseph Bazalgette, C.B., C.E., on the Sewerage of Brighton. It is preceded by the following retrospect of the results of some of the methods which have from time to time been suggested and tried for the better ventilation of the sewers of towns:

The removal or treatment of the gases resulting from decomposition in sewers in an inoffensive manner is a subject which during the last half-century has received much consideration. When in 1850 I was conducting experiments on the ventilation of the sewers of London, I had the advantage of consulting with that eminent chemist, Professor Faraday, who had previously given much attention to the subject, and who, in his evidence before a Parliamentary Committee as early as 1834, had expressed the opinion that it was beset with great difficulties. Subsequently I visited some of the mines in the north of England and in Wales, in order to see how far any of the modes adopted for their ventilation could be applied to the better ventilation of sewers, and I became acquainted with most of the suggestions which have been made for otherwise dealing with the gases generated in sewers.

In 1858 a Committee of the House of Commons, consisting of Lord Palmerston, Lord John Russell, Lord John Manners, Sir Benjamin Hall, Mr. Robert Stephenson, and Mr. Tite, directed me to make experiments on the effect produced by extracting and burning the gases of sewers by means of furnaces. Those experiments were conducted with the furnace in the clock-tower of the Houses of Parliament, and I subsequently gave evidence before that Committee, to the effect that in the immediate neighborhood of the furnace the indraught was found to be very strong, but that, whilst the supply of air was drawn with great force from the sewer inlets close to the furnace, the air current produced in the sewers at a short distance from the furnace was scarcely perceptible. The Committee of the House of Commons reported that, although such a process might be advantageous to sewers within a short distance of the furnace, it could not be successfully applied to any wide range of sewers, on account of the number of openings which unavoidably communicate with them, the nearest of which to the furnace would supply it with atmospheric air, whilst the gases in the further part of the sewers and house-drains would remain unaffected by its action.

In a mine there is but one downcast and one upcast shaft, and all the air brought into the mine at the downcast shaft can be directed and conducted at will, and discharged at the upcast shaft after it has passed through the whole length of the various galleries; whereas, in an ordinary system of town sewers, provided with inlets for the admission of water at every house-drain, gully, and branch sewer connection, the beneficial effect of furnaces, fans, or air pumps, becomes limited to a comparatively small area; but wherever furnaces exist in the neighborhood of sewers, it is nevertheless desirable to connect them with the sewers. In long lines of intercepting and outfall sewers, which have no branch connections or openings along their route, furnaces have been and may be used with the same beneficial results as in mines.

In 1866 Dr. Miller, F.R.S., and I conducted a series of careful experiments on the effect of ventilating sewers through charcoal, which extended over a period of twelve months and embraced a large draining area. The sewers were cut off from all other means of ventilation, except through charcoal trays of various forms fixed in the ventilators. We found that whilst dry charcoal is an efficient means of deoderizing and disinfecting sewage gases, its introduction into the ventilators produced a sensible retardation of the current of air in the sewers, and the carbonic acid in them was increased on an average of our experiments

from .106 to .132 per cent., and the mean temperature in the sewers was thereby raised from 50.8° to 56.2°. The beneficial effect of charcoal is, moreover, considerably reduced by moisture, and it therefore requires renewal at no very distant periods, varying according to the state of the atmosphere. Charcoal may be introduced with advantage into such ventilators as are the cause of any special annoyance; but, as they retard the current of air, their number and area would, if generally adopted, have to be increased to an extent which is for many reasons undesirable.

Shafts connected with the sewers and carried through lamp-posts in the streets, or to the tops of adjoining buildings, away from the chimneys and upper windows, might in many cases be so constructed as to ventilate the sewers efficiently, provided they were sufficient in number and in the area of their openings. But there is frequently much difficulty in obtaining the necessary consent for ventilators up the sides of houses on account of their having to be placed on private property.

The use of sulphurous acid and chlorine gas placed in ventilating shafts, and various other chemical or mechanical antidotes, have been attended with more or less beneficial results, and most of them may, under favorable circumstances, be applied in particular places with advantage; but all these modes of treatment require such constant attention and frequent renewal that they thus become liable to failure.

In order to prevent the evolution of noxious gases from sewage, the great object to be attained is its dilution and rapid removal, before decomposition has set in, by a copious supply of water, through sewers having sufficient falls to prevent the accumulation of deposits in them. Where these conditions cannot otherwise be sufficiently secured, the sewers should be kept clean by periodical flushing. Road detritus, if allowed to enter and deposit, in the sewers, will accumulate and precipitate with it much of the sewage which otherwise would not deposit. The efficient scavenging of the surface of the roads and the interception of the detritus washed off them during heavy rains by properly-formed catchpits, are therefore essential to the maintenance of clean sewers. Macadamized chalk, or gravel roads, especially those having steep inclinations, require particular attention in these respects. In 1878 there were in the metropolis 1,700 miles of roads, of which about 1,000 were macadam or gravel, and from the surface of the whole were removed in one year over 600,000 cubic yards of detritus, at a cost of about 1s. per yard; whilst about 100,000 yards were removed from catchpits under the gullies, at a cost of 2s. 6d. per yard, and 20,000 cubic yards were taken from the sewers at a cost of about 25s. per yard. Thus it will be seen that effective scavenging and the construction of proper catchpits are economical as well as being advantageous to the condition of the sewers.

There are few who will not now recognize that the removal of the refuse of large towns by water is so vastly superior to any other known method as to have caused it to be an essential in these days of civilization and refinement. But the underground carriers must be freely ventilated or the gases generated in them will escape into the houses, where, being shut up and but slightly diluted with atmospheric air, they are inhaled day and night, and become injurious to health, and dangerous. It will be found upon close investigation that in the great majority of cases where persons have suffered from the effect of sewer gases, the mischief has arisen from defective house drainage and not from the public sewers. Every house drain should be formed of stoneware pipes, laid with sufficient fall to prevent the accumulation of deposit, and ventilated from its upper end to the roof of the house, but very few are so ventilated.

The gases escaping from efficient sewers ventilated on to the surface of the roads may nevertheless, in certain states of the atmosphere, be offensive in the immediate neighborhood of such ventilators, and although no universal system of ventilation has yet been discovered which can be always applied without any inconvenience, some satisfactory mode may in every case be selected, according to the varied conditions of the localities to which it has to be applied. Attention to the foregoing principles of construc-

tion and maintenance of the sewers will very materially promote their ventilation without offense or injury.

At a meeting of the Yorkshire Associations of Medical Officers of Health, held in Doncaster in June—

Mr. B. S. Brundell, C. E., read a paper on "Ventilation of Sewers." He said the question of the ventilation of sewers was by no means easy to treat in an interesting manner, and still more difficult was it to make the subject instructive, as so much had been already written and said on the subject. He would, however, endeavor to give a practical turn to the subject. It might be taken as clearly established that if the sewers of our towns were constructed with adequate self-cleansing "falls," and with proper flushing arrangements, and if at the outfall a free discharge of sewage could be secured at all times, there would not be much need for ventilation; for there would be no foul matter in the sewers out of which to create what is commonly called sewer gas. But, unfortunately, the great majority of towns were so situated that the sewers could only have gradients with small "falls," and too frequently the outfall was obliged to be either partly submerged, or, as in the case of pumping works, at certain periods inoperative, and hence sewage was stagnant for hours near the outfall, or moving so sluggishly that decomposition was set up, and sewer gas resulted. The question, therefore, arose how this could best be got rid of. The mode of ventilation of sewers which met with most favor was that of open gratings on the surface of the streets, and those had been found effective. In Leeds, and in some other towns, the gully gratings were now made to act as ventilators, the traps formerly used being removed. He had grave doubts as to the wisdom of leaving a place of escape close to a house or a shop door. Some openings emitted much more sewer gas than others; and it was therefore not only necessary to provide ventilation, but to ensure a current of fresh air. The openings consequently should not only be numerous, but well placed for the purpose—in fact, a constant interchange between the outer air and the sewers should be aimed at. Where there was a tendency for the gas to travel up the sewers, flap-valves should be placed so as to stop the upward current. No doubt much could be done by the owner of a house in the construction of such connections as would obviate the risk of sewer-gas finding its way into the house; but if the main sewers were properly ventilated the householders' precautions would not be nearly so necessary as they were at present. Another mode of ventilation which had been much advocated was that of exhaustion by connecting the sewers of a town with the furnaces of steam boilers; but this necessitated a peculiar construction of sewer which would allow of the air being drawn from the sewers by the furnaces; and it was not clear what length of sewer could be so exhausted. Moreover, the furnaces of boilers were not always at hand. Still, no doubt, the principle was a good one, and he had tested it with success. The experience of Brighton was not very encouraging in this direction; and anything like the application of this principle of ventilation to the sewers of a town could not, he thought, be entertained. Ventilation by means of pipes carried up the chimneys of houses was sometimes adopted, terminating with an exhaust ventilator, and which had been successful in some cases; but it should be carried out with great care, for in some places this system had been traced as the cause of blood poisoning. He would urge, as one conclusion to which he came, that main sewers should be systematically flushed; and the outfall of main sewers, as a rule, should have falling-doors, so as to prevent wind blowing up the sewers.

Mr. Masters read a paper on "The Circulation of Air in Sewers." Sewer-construction, he said, had been broadly distinguished by the terms "sewers of deposits" and "sewers of suspension." The former involved a system of flushing; in sewers of suspension a continual flow and circulation of air were provided. They were told on the best authority that sewers to be self-cleansing must have a certain grade, and he quoted from a table of inclinations, which gave the grade of a self-cleansing 15-inch drain at a fall of 1 in 250. He believed the most effectual means of creating a good current of air and ensuring ventilation and thorough cleansing of the sewers was by a constant stream through the whole length of the sewers (instead of an occasional one), at

a velocity of not less than 3 feet per second. It had been proved that the air would follow a stream traveling at 2 feet per second, in preference to rising to the highest point of the sewer. Any system of sewerage which provided for the removal of the sewage at so slow a rate that sewer gas was left behind must be imperfect.

Dr. J. M. Wilson read a paper on "The Ventilation of House Drains." He said the house system of drainage should be provided with means of cutting off the waves of sewer air, or at least of giving them an exit in a way harmless to the house inmates. He wished chiefly to elicit an opinion as to how far some principles of drain ventilation were satisfactorily answered by the requirements of the Local Government Board in their recent by-laws applicable to house drainage. That air from the house drains or sewers was in its effects injurious to health, and capable of originating definite forms of disease, they, as medical officers, had too many opportunities of confirming. These connections were as a rule very defective, He proceeded to discuss the by-law to which he referred. If, he said, it could be satisfactorily agreed that the plans proposed answered the theoretical requirements of the laws governing the action of gases, and—as their adoption was already being proved to be—more effectual than any previous practice in shutting off all air from the drains from entering the house, then he thought they might safely leave it to their engineering friends to smooth away any practical difficulties. To sanitary authorities and the public they could safely recommend a system which satisfied the principle of drain ventilation, and the adoption of which they might reasonably anticipate would rid us yet more of the class of diseases caused by what had been called aërial sewage.

The Chairman remarked that the question of the correctness of the germ theory underlay the discussion, and an important point to be considered was whether sewer-air was capable of carrying germs of disease.

Dr. Whitelegge, in referring to the first paper, remarked that if the ventilators to sewers were constructed sufficiently close to each other, it would be impossible for poisonous gas to accumulate in sufficient quantity to prove injurious.

Dr. Himes said that in the whole course of his experience he had never met with a case in which sewer-gas had produced specific disease. If it were true that sewer-gas did cause specific disease, medical officers would find themselves in the difficulty of having to condemn the present system of drainage in large towns. But where was the proof that sewer-gas was the cause of disease? A case of typhoid fever was found in a house, and an examination showed that the house was in direct communication with the main sewer. But so were thousands of houses in which there was no fever. In Sheffield there were acres upon acres without sewers of any kind, and so there were hundreds of villages, yet they did not find these districts any better off in respect of zymotic diseases. In his opinion it was matter for regret that sanitary authorities gave almost their entire attention to the causes of zymotic diseases, instead of endeavoring also to prevent, as they could in a great measure, that frightful scourge consumption, and probably also the large number of deaths from bronchitis and pneumonia, which were largely attributable to the same causes. But to return to the question of sewer-gas, in looking through the death-rate of his borough he did not find in those seasons in which the decomposition of sewer matter was most active that so many deaths from zymotic diseases were registered. In Croydon, where typhoid fever had been more or less prevalent, the bad smells found in many of the houses were assumnd to be sufficient proof that sewer-gas was the cause of the fever. He did not think that was sufficient proof. His experience did not fortify the second-hand opinions which had been laid before the meeting relative to sewer-gas.

Dr. Wills suggested that water-spouts as conductors of sewer-gas were preferable, at least from an æsthetic point of view, to open shafts over which one had to walk.

Mr. Hodgson, C. E., remarked that it was a fundamental error to suppose that a certain amount of velocity in a sewer was all that was necessary to carry off sewer-gas. In connection with sewer ventilation, of whatever description, there must also be a system of cleansing.

Dr. Whitelegge urged these points for the acceptance of the meeting—namely, that sewer-gas did not mean merely a

mixture of well-known chemical gases; that sewer-gas did not necessarily give off a bad smell; that it was not necessarily heavier or lighter than the surrounding air; and that it was deleterious.

The Chairman said he apprehended that sewer-gas was the sum total of all the vapors proceeding from the contents of sewers—nothing more nor less, in fact, than the results of decomposition, and varying at different seasons and in different temperatures, and in proportion to the contents of the sewer. In his judgment, a large amount of the most dangerous and pernicious gas was almost odorless. As the effect of its action, people were deprived of a great amount of the air they breathed. According to the teachings of the last fifteen or twenty years, all organic compounds in a stale condition were prone to excite other stale conditions in any organisms with which they came into contact, and the admission of these unstable compounds into our bodies was therefore, according to modern science, a fertile scource of danger, and if they did not set up changes or actions in our bodies it was because we were in a condition to resist them. What was true of zymotic diseases was also true of noxious diseases arising from these causes. Then in these sewers we had the undoubted carriers of these noxious germs. By means of the connection between houses and sewers the infectious diseases of one house were carried to other families. And we had these diseases let into our houses in every possible way—by bathrooms, by water closets, and by other ways—and the only way of escape was by complete isolation from our neighbors. He frequently advised his friends to open out all dead ends of pipes and drains, so that there shonld be free and perfect exposure to the air. If we could have impervious floors and walls with, practically, open ditches for drains, we should best stave off disease; those who lived in villages would know quite well that the open ditch was far less offensive than a good many of our more expensively constructed sewers.

FAILURES IN RAILWAY EMBANKMENTS.

By JOHN WILLIAM DRINKWATER HARRISON, Assoc. M. Inst. C.E.

From Selected Papers of the Institution of Civil Engineers.

The unusual difficulties encountered by engineers during the last five years in the construction of railway earthworks, have been to a great extent attributable to the abnormal state of the weather during that period. In no other class of work can a completely successful result be anticipated with so little confidence, and a satisfactory solution of the difficulty still appears remote. From the great outlay which is often necessary to restore the ground after an extensive slip, it may be questioned whether greater precautions, and consequently increased expenditure during construction, are not desirable. Probably the material does not exist which, if thoroughly freed from the presence and action of water during the process of construction, would fail to form a permanently stable structure; the value of the forces of cohesion and friction depending so largely on this condition.

The separation of the sound or dry material from the unsound is a matter of the first importance, and sufficient attention is not generally given to it. There are differences of opinion as to what constitutes unsoundness, and a practical definition of it is by no means easy. The process of separation frequently involves additional labor on men who require great supervision; land whereon to deposit the soft earth is not always available, and the common practice of casting it out on the sides of the nearly finished bank is unsatisfactory. Where burnt ballast is required, the best method is to light fires adjacent to the cutting, and to burn the wet material. Considerable importance is believed to attach to this point, as the commencement of slips of a serious nature has been traced to the admission into an embankment of two or three wagons of "slurry."

On the recently constructed Nottingham and Melton railway several serious slips occurred. Some idea of the character of the material may be formed from the fact that one-fortieth of the excavations was burnt into ballast for use on temporary roads only. Great care should be taken to drain transversely the water which collects in the ballast so used, otherwise, the temporary road sinking to a lower level than the bank on either side, a trench retaining water is left in the center of the embankment, which is a fruitful source of trouble. The rule adopted on the above line in forming the slopes of earthworks was:

For cuttings and embankments under 25 feet deep, slope 1½ to 1.
For cuttings and embankments above 25 feet and under 40 feet deep, slope 1¾ to 1.
For cuttings and embankments above 40 feet, slope 2 to 1.

Any attempt, however, to arrive at a definite angle of repose for such material is not likely to be successful, several of the slips having assumed a slope of about 8 to 1.

Experience fixes 30 feet as the limit of height to which it is advisable to carry a bank of blue clay; the necessarily slow progress made in higher banks exposes the earth on the leading face so much to atmospheric influences, that, in a bad season, the slope is continually in a soft condition, and is an unfit foundation for the reception of any material. To avoid this evil, by making more rapid longitudinal progress, several of the heavier embankments were formed in two lifts. If, however, the season is a good one, it is better to tip a bank to the full height in the first instance. In tipping it at a lower level there must be a sufficient allowance for settlement, otherwise the base on which the higher lift is to rest will be too narrow. Since this settlement varies in different soils from 2 to 6 inches in the foot, the difficulty in determining beforehand what allowance is necessary, renders this contingency of a narrow base a not unfrequent occurrence, and obviously necessitates beveling the extra width on the slopes of the lower lift, which is always to be avoided. Then again, the surface of the lower bank being in an uneven state induces the collection of water and consequent saturation of the work.

On sidelong ground, in pasture land, the grass affords a sufficiently smooth surface to induce a movement in the bank. The author believes that a system of surface digging to a depth of 9 inches is preferable to the formation of benchings. The latter need careful drainage, and when cut at right angles to the center line of the railway, the mound formed from the excavation of the benching, being composed mainly of light turfy soil, gives way under the weight brought on it, and so not unfrequently causes a failure extending into the bank. The author recently had occasion to widen an embankment for siding purposes; one part of the slope of the already formed bank was benched, the other surface was dug, and it was found that the latter stood better than the benched portion. In this case, however, though great care was taken to drain the benchings when formed, a settlement may have taken place in the old bank, causing an accumulation of water in the benchings.

Desirable as it undoubtedy is to ascertain, by borings, the nature of the material to be excavated before commencing operations, little or nothing can be learnt in this way as to the probability of the subsequent occurrence of slips; nor does it follow that a material which will stand well in cutting will form an equally good bank, and *vice versa*. The excavation from a cutting on the Nottingham and Melton railway, which was deposited in a spoil bank, stood well at a slope of 1 to 1 or less; whereas the cutting whence it came gave no little trouble, though its slopes were flattened to 2 to 1. In this case the presence of "backs" caused the trouble in the cutting, the process of excavation and removal obviating this danger in the bank.

Slips are more frequent in autumn, after a dry summer, than at other seasons. The probable explanation of this is that the cracks formed by the sun collect the rain, and where these cracks occur near weak points of the bank, the bank fails. To prevent, as far as possible, the occurrence of cracks, great care was taken to obtain a good growth of grass. It has been suggested that a layer of burnt ballast 6 inches thick, placed beneath the soil in which the grass is sown, would not only be useful for drainage, but also

protect the clay from the effects of the sun.

The slope assumed by plastic clay, when first tipped, seldom exceeds 1¼ to 1. Now although the slope which is ultimately to be given, and which is considered necessary for the stability of the work, may extend to 2 to 1, for reasons of supposed economy in working and to give time for any extra settlement beyond that allowed for, the embankment is usually left at the steeper slope for periods extending in some instances to several years. During this time, it appears to the author that, allowing the more extended batter to be a correct estimate of what is necessary, an excessive strain is placed on the work. The slips which occur while the bank is in this condition are sufficiently frequent to lend some force to this argument. Though these slips may not be of a heavy character, nor even extend beyond the ultimate slope line, it is noticed that they remain weak points in the work and occasionally lead to serious disturbance. To remedy this, it seems desirable that the process of forming the slopes should be carried on as nearly as possible simultaneously with the construction of the body of the bank. The objection to this system on the score of expense is not a serious one; and alllowance for further settlement might be made by slightly increasing the width of the formation; indeed, in ground of this character, a somewhat extended formation may be beneficial in other ways. The additional outlay in land in most districts is hardly worth consideration, the main question in cost being the increased quantity of excavation necessary.

In treating slips after their occurrence two methods were mainly adopted:

1st. The toe of the slip was burned into a compact mass of ballast, the width at the base varying from 8 feet to 20 feet or more. This retaining wall, for such it virtually was, having been formed, the foot of the slip was weighted as far as possible, and the slope was left concave where practicable, having a versed sine one-thirtieth of its length. The foundation of the ballast heap was 2 feet below the original surface. In no case did this wall of ballast give way, though in several instances the slip rolled completely over it, and a fresh heap had to be formed at a greater distance from the line. As the circumstances were exceptional, any details as to cost would be misleading; but it may be stated that 1 ton of coal was sufficient to burn about 10 cubic yards of ballast.

2d. Trenches were cut through the slips at right angles to the direction in which the ground was moving; the width of these trenches varied from 2 to 9 feet, and having been carried 18 inches or 2 feet into the solid ground below the line of the slip, they were filled with stones, the whole of the timbering necessary for their excavation being, generally speaking, left in. This is obviously a costly process, and was only adopted in extreme cases, where the slips were delaying the opening of the line. In excavating the trenches it was noticed that but little water was tapped at a lower level than 3 or 4 feet below the surface. That they must be regarded as counterforts to strengthen the slips more than as means of drainage was shown by the fact that several weeks after their construction the surface of the bank 3 feet away from the trench was in a soft, boggy condition. Regarding them, then, simply as counterforts intended to strengthen a moving mass of weak material, it was thought that to carry them completely through that mass would defeat the purpose for which they were formed, and allow the slip, or succession of slips, to continue their course between the walls. It was found that carrying them about two-thirds of the way through the slip effectually checked its progress, and it seems probable that a less distance than this would have sufficed.

In all cases, where the trenches extended to the back of the slip, there was no great quantity of water. The cause of the majority of the failures appeared to be the inability of the material to support its own weight, consequent on the quantity of water with which it was charged; that this water is held in suspension for a great length of time appears probable, and the fact that the heaps of ballast over which the slip had rolled were found, when opened out, to be in a dry and dusty state, shows that the plastic nature of the clay prevents gravitation, and the process of evaporation in

a deep bank must be slow. More than once where the base of the slip was on the same level as, and extended to the bottom of, the ordinary open side ditch, a pipe-drain filled with rubble was substituted with advantage.

CO-EFFICIENT OF SAFETY IN NAVIGATION.

By PROF. W. A. ROGERS.

Abstract of a Paper before the Society of Arts, Boston.

PROF. ROGERS first referred to and explained the use of the co-efficient of safety in the calculation of the size of timbers used in building from the experimentally-determined breaking load. He then proceeded to discuss the errors to which observations to determine the position of a ship at sea are liable, with the object of finding how wide are the limits of these errors, so that it might become possible to find a co-efficient, as in the case of the timber, by which this error might be multiplied to secure absolute safety, as far as safety depends upon human means and exertions.

This important question of how large an error is liable to enter into the determination in a ship's position appears to have been almost wholly neglected, at least in so far as published discussions are concerned. It is not referred to in the extensive press utterances nor in the Court of Inquiry which followed the disaster to the steamer Atlantic. In the whole forty-three volumes of the *English Nautical Magazine*, in the *British Admiralty Law*, especially in the new code adopted in 1849, in the *Wreck Register*, published annually by the British Board of Trade, nothing appears upon this subject. If navigators proceed upon the supposition that they can with certainty obtain their position within one mile, to say nothing of 300 feet (as reported to have been stated by Capt. Williams of the Atlantic), the wonder is not that so many wrecks occur but that more do not occur. Yet the general testimony of sea captains in answer to inquiry is that one mile is the ordinary limit within which the co-ordinates of a ship's place can be determined.

By tables of statistics of the shipping of Great Britain since 1838, Prof. Rogers then showed that there has been a large increase of disasters in proportion to the whole number of vessels, a fact which justifies a new discussion of the whole problem of wrecks and their causes. In the following investigation it is proposed to examine only those causes of wrecks which in a measure seemed to have escaped attention in official investigations. These are:

1. Wrecks produced by causes clearly beyond human control.
2. Wrecks resulting directly or indirectly from over-insurance.
3. Wrecks caused by the deviation of the compass.
4. Wrecks caused by errors of observation at sea.

The first inquiry is the most important one, as, if we can find the number of wrecks from causes beyond human control, we may thus ascertain how many are *within* human control.

By an examination of the records of the Court of Inquiry for twenty years it appears to Prof. Rogers probable that at least seven out of ten wrecks occur from preventable causes.

Under the second heading the following facts may be given:

1. It is certain that more insured than uninsured vessels are lost.

2. In 1868 there were in the Baltic 220 Swedish steamers, and, in 1867, 215 British. Of these 3 Swedish and 17 British were lost. From 1857 to 1867 the ratio is 10 British to 3 Swedish. The British vessels were insured, the Swedish were not.

3. Admiral Halstead, Secretary of Lloyds, in a speech before the United Service Institution, said: "The remedy for shipwrecks,—what is it? I do not pretend for one instant to be able to provide a remedy, and I do not know anybody who can undertake to say what is a remedy for shipwrecks, but I will tell you this. If I could go on the Stock Exchange to-morrow morning, and, by holding up my hand, put a stop to all ship-

wrecks upon the coast, it would be a question how I could get safe with life off the Exchange. When I put that question to him (Lloyds), he said: 'It is perfectly true, you would stop our bread.'" We have here the highest authority for saying that the whole question of insurance involves more or less of fraud, and that ships are purposely wrecked. In 1866 Thos. Berwick was convicted for being accessory to the destruction of ships owned by T. Berwick & Son. On his trial he confessed to having destroyed no less than nine vessels in the course of twenty years. The case of the Dryad and the Harlequin in 1837 shows that in those days at least the question of insurance had a very definite bearing on that of wrecks.

On the third heading the speaker said that his investigations were far from complete or satisfactory on account of the difficulty of obtaining reliable data. Prof. Rogers then discussed the discovery of the variation of the magnetic needle from the true north, and the amount and the secular changes in amount of this variation. The amount of this variation could be determined and corrected for, but the problem of the deviation of the compass on ship-board is complicated by other effects. An iron ship, or one having any considerable proportion of iron in its construction or cargo, becomes a great magnet by the action of the earth's magnetism, and thus disturbs its own compass needle. In iron ships this deviation often amounts to 50°, thus rendering the compass useless, unless some compensation or correction is applied. This subject was first investigated by Capt. Flinders in 1811. The polar expedition of 1818 fully confirmed Flinders' experiments. The next important work was that of Barlow, which led to Airy's method of correcting the deviation by swinging the ship and correcting the deviation by permanent magnets or soft iron placed in suitable positions near the compass. But the most important discovery was by Dr. Scoresby, who found that the ship was itself a great magnet. In his voyage in the Royal Charter, to test his theoretical conclusions as to the changes in the magnetism of the ship in different positions, localities and other conditions, he found them verified. He also found a sensible difference in the variation before and after steam is up in the boilers of a steamer. The effect of the heel of the ship has recently been investigated, and also the change in magnetic condition of the ship after launching, some three months being required for anything like a permanent and regular condition to be attained. But even with all these studies and the corrections arising from them, there may often exist unknown variations of very considerable amount, yet the London Compass Committee, as late as 1869, declare that very few ships are lost from this cause. What shall be said of ships that are never swung, and whose masters know nothing of the laws of variation? The loss of the City of Washington is the best refutation of this statement.

The fourth topic was next considered. Under the offer of a reward of £20,000 by the British Admiralty, Morin, Maskelyne and Huygens made attempts to produce methods for determining the longitude at sea within thirty miles. The method of the latter was to use watches, determining the difference in longitude by the difference in time. This method was unsuccessful with Huygens, owing to the variation in the rate of the watches used with temperature changes. But Harrison finally produced a chronometer which, by the excellent workmanship of its construction, gave results within the required limits, and this method has since been generally adopted. Even in observatories fitted with the most delicate appliances the difference of longitude is difficult of exact determination. For instance, the difference in longitude between the Greenwich and Paris Observatories in 1755 was supposed to be 9′ 16″; in 1830 it was found to be 9′ 21.5″, a difference of 5.5″, or 1¼ miles. The range between Greenwich and Brussels is ten miles. Several determinations by different methods by Dr. Bowditch upon the long. of the Old State House at Boston differ by 2.6 miles, and the mean is in error by ¼ mile. Yet all these are hardly comparable with any single observation on land or sea. Tables of determinations of the longitude of Washington show a range of 1½ miles, and the mean is in error 1.4 s. These figures illustrate the difficulty of the determination even under the most favorable circumstances.

For the determination of longitudes at

sea, two essentially different methods are used.

1. By "Lunar Distances," occultations and eclipses of Jupiter's satellites.

2. By chronometers, assuming their rate at the beginning of the voyage to remain constant.

The latter method has been for a long time regarded as far more reliable than the former. To compare the two afresh, Prof. Rogers presented elaborate discussions:

1st, of the results of a large number of land observations at fixed stations, and also of sea observations, with the following general results:

For fixed observations, comparing with the mean result at any station, we must expect an error of 1.5 m., with a range of 5.2 miles.

For fixed observations, using the moon's tabular places, an error of 3.1 m., with range of 12.9 m.

For lunar distances, with sextant on land, an error of 10.21 miles, with range of 24.2 miles.

For lunar observations at sea these quantities should be doubled.

2d, of a large number of chronometer observations, including series from the Greenwich Observatory; from the chronometers of the Cunard Steamship Company; by Prof. Bond of Harvard College, in 1849 and 1850, and many others. As a result of this discussion, Prof. Rogers states that taking the mean of the value given by Mr. Hartnuss, $=0.98$ s., and that found by himself, $=0.48$ s., we have for the average daily error of rate of all these chronometers 0.73 s. At the end of twenty days, therefore, the navigator must expect from his chronometer alone an error of 3.6 miles. We must look out for an error of $3.6 \times 3.2 = 11.5$ miles (when 3.2 is a factor of safety deduced from the discussion), and the amount of his error may prove to be at least twice this quantity of twenty-one miles, all on the supposition that he has an average chronometer, as this is independent of the error of observation which must still be added.

Prof. Rogers then turned to the final question: how near is it possible to find the place of a ship at sea by astronomical observation? Confining himself to the usual method, viz., the measurement of the altitude of the sun with a sextant, at a given time before it comes to the meridian, for longitude, and of its culmination for latitude, he enumerated some of the errors to which this method was liable. These are:

a. Instrumental errors.

b. Error in noting time. This is never taken closer than 1s. Multiplying by the co-efficient 3.2, previously found, gives an error amounting to nearly one mile.

c. From imperfect sea-horizon. This may amount to several miles.

d. From the use of approximate data. In ordinary practice the use of approximate corrections, and the lumping together of several of these, may easily cause an error of five miles or more.

e. From latitude of ship and time of observation. These may be very large, and for the most part escape the attention of the navigator.

f. From the error in the estimated run of the ship between the morning and noon observations. It is impossible to give any definite estimate of the magnitude of this error, but it is likely to exceed all the others combined.

In addition to these, Prof. Rogers gave an investigation of errors of sextant observations in general, from which he deduced as an estimate for sea observations an average error for latitude of about 1′, and for time of about 6 s.

The German Ironclad "König Wilhelm."—In the early part of last month, this vessel made a six-hours' trial trip on the completion of her repairs. She was built in 1868 by the Thames Iron Works Company, from the designs of Mr. E. J. Reed, at that time Chief Constructor to the British Navy; and she was when launched the most powerful ironclad in the world. Commenced to the order of the Turkish Government, which could not complete its payments, the hull was purchased by Prussia, and finished to her order. Although now surpassed in strength and weight of armament, the König Wilhelm is a very formidable vessel. She is 356 ft. long, and 60 ft. 6 in. beam, with a displacement of 9757 tons. The engines are 8000 horse power indicated, giving a speed of 14½ knots. In addition to the repairs rendered necessary by the collision with the Grosser Kurfürst, the engines have been improved at a cost of £7634, and the armor has been increased.—*Engineering.*

GORDON'S FORMULA AND RADIUS OF GYRATION.

By Rd. RANDOLPH, C.E.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

ALTHOUGH civil engineers and bridge builders have generally adopted a certain formula for the construction of columns and other compression members of iron structures, it is doubtful whether any of them could give a satisfactory reason for the employment of some of the quantities which are used. When Gordon announced a formula based upon a long series of careful experiments by Hodgkisson with columns having a solid rectangular cross-section, it was adopted with full confidence, from the fact of its having so practical a foundation. But when it was attempted to apply the same to columns having an irregular cross-section, it was seen that in such cases it was no longer applicable; and it became necessary to substitute for the least diameter some other factor. From what considerations this factor was determined, it seems that all the authorities are silent. Professor Rankine, whose work on engineering is held in such high repute, uses language on this subject, so different from the exact statements of science, that it would indicate a want of confidence on his part in what he propounds. After giving certain modifications of Gordon's formula, he says—"but from the nature of the calculation these results must be regarded as rough approximations only." And in laying down the one which has been so generally adopted by practical engineers, he says—"but in many cases *it may be more satisfactory* to take into account the least radius of gyration of the cross-section."

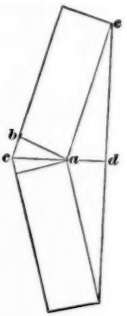
To one who cannot have a satisfactory feeling about any formula of which the data are not determined either in practice or theory, it becomes necessary to analyze it and to inquire how the physical laws have been applied.

The first question which suggests itself in an inquiry into the Gordon formula is—why does a column bend, supposing it to be perfectly straight and the force to be applied uniformly in lines parallel with it axis? To this question there can be but one answer; the difference in the elastic force of different parts of the material. If this were absolutely homogeneous, the only effect of the pressure would be to increase the diameter and to diminish the length; as there would be a simultaneous and equal yielding at every point within the limit of elasticity. If, however, owing to the irregular resisting power of the material, one side becomes shorter than the other, the column will assume those curves and deflections necessary to maintain the parallelism of its sides. As the inequality of compression will not be confined to one locality or to one side, it may take any form between a regular curve and a spiral. As soon as a deflection is determined by unequal compression, and the forces of action and reaction form an angle with each other, a resultant force ensues at right angles with the straight line between the two ends of the column, and which reaches a maximum at the point of greatest deviation from this line. In the great majority of cases this greatest deviation will be at the middle of the column; and being the least favorable for its strength, this condition should be the one contemplated in the formula which provides for lateral yielding. As the amount of deviation from the straight line depends upon the difference in the length of the two sides measured in straight lines from the point of deviation to the ends, the curves in the column on either side of this point do not enter into the question; for they give rise to minor lateral strains only, and the provision for the maximum strain at the middle point applies to the whole length of the column. The case will, therefore, be considered as a simple deflection at the middle; the difference of length of the two sides being the departure of two diameters from each other at the point of deflection.

It is the object of the formula to embrace a coefficient, determined by experiment, which shall represent the differ-

ence in the ratios of shortening of the two sides by the compressive force of the weight to which the column should be subjected when bending is not considered; and to determine from this the deviation, the resulting lateral strain and the necessary addition to the quantity of material in order to resist this lateral strain. If the Gordon formula, applied to solid rectangular sections is correct, the coefficient there used must express this difference in ratio of compression, as will be seen by deducing the formula from the premises just referred to.

Let C denote the difference in ratio of compression and L the length of the column. Then C.L will be the difference



in the length of the two sides. Let D denote the least diameter. One-half of C.L is the side *cb* of triangle *acb;* and as this is similar to triangle *ade*,

$$\frac{cb \times de}{ab} = ad. \quad \text{That is,} \quad \frac{\frac{C.L}{2} \times \frac{L}{2}}{D} = \frac{C.L^2}{4D};$$

which is the deviation.

As the angle of deflection in such cases are so small that the base and hypothenuse of these triangles are practially equal, they are so considered in this discussion.

If a force were applied at *c* at right angles to *de*, one-half of the resulting force would be resisted at each end of the column; and each would be in the same proportion to half the original force, as line *ae* to *ad*. Therefore half the lateral strain is in the same proportion to the force at either end of the column as the deviation is to half the length. That is,

$$\frac{W \times \frac{C.L^2}{4D}}{\frac{L}{2}} = \frac{W.C.L}{2D}$$

which is one-half the lateral strain; W denoting the weight per square inch on the end of the column. The condition of this strain is the same as if the column rested in a horizontal position on a fulcrum at the middle and had $\frac{W.C.L}{2D}$ suspended from each end, except that it must be considered to be at the same time under longitudinal compression. So that the effect of the weights would be to compress still more the material below the neutral axis at the middle and above it at the ends where they receive the first compression, both, however, requiring the same expenditure of force. The strains to be resisted on either side of the neutral axis are parallel with the column, tending to separate or compress the particles in that direction with a rapidity in proportion to their distance from the axis; which gives them a capacity of resistance in the same proportion; just as a resistance applied to a lever is efficient in proportion to its distance from the fulcium.

The resistance to separation or compression of the particles on either side of the neutral axis resemble resistances applied to different points along the short arm of a bell-crank, representing the semi-diameter of the column, to a weight suspended from the long arm, representing the half length of the column. In the case of a solid rectangular section, the particles are disposed with uniformity along the semi-diameter; therefore their combined resistance is the same as if they were all located on a line half-way on the semi-diameter, or a quarter of the diameter from the neutral axis. And as this is supposed to be in the middle of the cross-section the condition is the same on the both sides. So that the force to be resisted by all the particles will have the same proportion to the force at the end of the long arm, as half the length of the column has to one-fourth of its diameter. That is,

$$\frac{\frac{W.C.L}{2D}\times\frac{L}{2}}{\frac{D}{4}}=\frac{W.C.L^2}{D^2}.$$

is the amount of force per square inch of section caused by deflection. Therefore each square inch of the section must be reinforced with sufficient material to resist $\frac{W.C.L^2}{D^2}$. And as W is the assumed strength of the material, $\frac{C.L^2}{D^2}$ is the additional quantity, and $1+\frac{C.L^2}{D^2}$ is the increased quantity to be substituted for each square inch of the original section. So that instead of the capacity being W per square inch, it is to be diminished, when bending is also to be resisted, to

$$\frac{W}{1+\frac{C.L^2}{D^2}};$$

which is the Gordon formula when C is substituted for the coefficient there given.

If the two ends of the column are square, and the surfaces between which they are pressed extend over the ends, and are formed of material equally resistant, any bending of the column would require not only a compression on one side of the neutral axis at the middle but also on one side at each end. At the middle, on the side towards which the column bends, the bending is a relief from the original compression and meets no resistance; but on the other side the particles on each side of the semi-diameter move towards it at the same rate that they move towards the pressing surface at each end, the two end pressures being equal to the one at the middle. Therefore the resistance in the case of square ends is to that of hinged or pin-bearing ends is as 4 to 2; and when only one end is square, 3 to 2. Rankine reports the coefficient of Gordon's formula in the case of square ends to be $\frac{1}{36000}$. If this is substituted for C in the above, we have

$$\frac{W}{1+\frac{L^2}{3000D^2}}$$

For pin-bearing ends the additional material must be doubled, because there is only half the resistance to deflection; which would make it

$$\frac{W}{1+\frac{L^2}{1500D^2}}$$

According to Rankine, however, Gordon requires the additional area in the case of hinged ends to be four times that for square ends. This proportion is contrary to any theoretical reasoning on the subject and leaves in doubt which one of the cases was determined by the experiments. In proposing a formula in which the radius of gyration is substituted for the least diameter, Rankine observes this same proportion in the additional area in the two cases; but without explanation substitutes the coefficient $\frac{1}{3000}$ by $\frac{1}{36000}$, 36,000 being the same as Gordon's value for W.

But however satisfactory may be the Gordon formula for solid rectangular cross-sections, the analysis just made shows that it cannot be correctly applied to irregular sections where the material is not uniformly disposed along the line of the diameter. The quantity $\frac{D}{4}$ would then have to be substituted by another; which would be that multiplier of the sum of all the particles on either side of the neutral axis which would give the same result as the sum of the products of the particles each multiplied by its own distance from the axis. As before, it is a statical question like that of the equilibrium of a lever under parallel forces. If one arm of a lever extends ten feet beyond the fulcrum and one pound is suspended from the center of each foot, the effect is the same as if ten pounds were suspended from the center of the arm. This would illustrate the solid rectangular section. If, in addition to the weight on each foot, ten pounds should be suspended at the end of the arm the effect would be the same as if twenty pounds were suspended at the point three-quarters of the distance from the fulcrum; which would illustrate a section having a stem with a flange at the end of it. Now these weights represent the particles of material in the section whose re-

sisting power is in direct proportion to distance of each unit from the axis.

With the view of the subject it is difficult to conceive how the radius of gyration, which finds its application only in dynamical questions, could have been introduced into the formula. To see the distinction, consider the example of a revolving wheel about a vertical axis whose speed is maintained or uniformly accelerated by the application of a constant force, such as a descending weight. On the principal of the lever each particle, by its inertia, offers a resistance to the force in proportion to its distance from the axis of rotation. If one of these particles is moved to a greater distance from the axis, its original velocity cannot be maintained without a greater expenditure of force than it required before; because this would require the same pressure through a long as through a short lever. But at the same time the particle must increase its velocity, if the general rate of rotation is to be maintained; which will require another increase in the force applied. If the distance from the axis is doubled it will require the mass, and consequent inertia, to be reduced to one-half in order that its original velocity or original rate of acceleration may be maintained with the same application of force. But as its velocity or rate of acceleration must be doubled in order to preserve the general rate, the half mass must be divided by two. That is, to say, that in order to preserve the conditions of motion of a revolving body or system, the mass of each particle must remain in proportion to the square root of its distance from the axis; while their combined influence will be expressed by the sum of the products of the particles each multiplied by the square of its distance. That distance from the axis which, being squared and multiplied by the sum of the particles, produces this result, is called the radius of gyration. And whatever changes are made in the mass or position of the particles in reference to the axis, the square of the radius of gyration must remain constant in order to preserve the condition of motion of the system.

But the question of statical resistance to tensile or compressive strains on either side of the neutral axis, resulting from the effort to bend a column, involves no other consideration than the number of particles or fibers and their average distance from the axis. To this question the radius of gyrations has no application whatever, and its retention in the formula will cause constant discord in all future attempts to obtain a true co-efficient derived from experiments.

It is evident that the quantity $\frac{D}{4}$ in the process which results in the Gordon formula must be exchanged for one which, being of the same value in solid rectangular sections, will be equally correct in all others. This might be called the radius of resistance, for the resistance is the same as if all the material were concentrated at the end of it. This quantity can be nothing else than the distance from the neutral axis to the center of gravity of that part of the section on one side of the axis: for a lever cannot apply all its forces to any fulcrum except the one of equilibrium.

Let the new quantity be denoted by R and substitute it for $\frac{D}{4}$. We will then have

$$\frac{\frac{W.C.L}{2D} \times \frac{L}{2}}{R} = \frac{W.C.L^2}{4D.R}$$

for the strain per square inch, and $\frac{C.L^2}{4D.R}$ for the increase of each square inch, and

$$\frac{W}{1+\frac{C.L^2}{4D.R}}$$

for the weight per square inch to which the column is to be subjected so that it may resist compression and bending both, both end bearings being square.

Supposing Gordon's co-efficient, $\frac{1}{30000}$, to be correct, the formula for square bearing ends would then be

$$\frac{W}{1+\frac{L^2}{12000D.R}}.$$

For other modes of bearing the addition to 1 in the divisor of W would be in the proportions before mentioned.

The value of c can only be expected to be sufficiently constant to ensure the er-

rors being confined to narrow limits and to enable it to serve the practical purpose for which it is employed. There is need, however, for more experiments, for it should be determined with more certainty than has yet been done.

ON SEWER GAS AS A FACTOR IN THE SPREAD OF EPIDEMIC DISEASES AND ON THE DIRECTION AND FORCE OF AIR CURRENTS IN SEWERS.

"Deutsche Vierteljahrsschrift für öffentliche Gesundheitspflege," for Abstracts of the Institution of Civil Engineers.

PART I.—By DR. SOYKA, of Munich.

THE author draws attention to the fact, that while England was the first country to introduce an improved system of sanitation, it was in Munich that the theory of the dangerous nature of sewer-gases originated; a doctrine which is receiving a considerable share of attention in Germany, the tendency being greatly to exaggerate the danger. According to this theory, the air contained in the sewers, on escaping into the streets and houses, occasions the spread of epidemic diseases. In England this doctrine is gradually taking the place of the favorite, but somewhat exploded, notion of infection by means of the water-supply. For, whereas formerly whenever any impurity in the water was detected this was at once made answerable for any outbreak of typhus or cholera: so now typhus, diphtheria, and other diseases of this type, are immediately declared to be caused by some faulty drain or water-closet. It is frequently not even considered necessary to prove that there has been any actual escape of sewer-gas, and no attempt is made to trace the possibility of any contact of the patient with such gases. The convenience of making the foul gas responsible often, indeed, hinders any proper investigation from being made into the impure gases in sewers, latrines, and other possible causes of infection. In considering the subject, all cases of sudden death or illness caused by inhaling similar places, may be left out of the question, for what is now to be dealt with is not sewer poisoning, but the spread of certain diseases, either of an endemic or epidemic character, which arise in consequence of the reception into the system of an organism, which there multiplies and becomes the germ of new cases of infection. For, while it is impossible to deny that long continued exposure to impure gases may cause a feeling of illness and discomfort, it is not pretended that the foul gas in sewers can give birth to the germs of typhus, diphtheria, &c., but only that such gases serve as the medium in which these organisms are suspended and conveyed to the patient. The author gives a table showing the mortality from typhus, or so-called "enteric fever," in a number of English towns, before and after the completion of the sewering; and some special tables relating to Croydon, showing a spring and an autumn maximum in the cases of zymotic diseases. Dr. Buchanan is quoted, and blamed for contenting himself with the fact that the infected houses, in the latter case, were connected with the sewers, without making any attempt to prove that the sewer-gas escaped into the dwellings. He stated, indeed, that no smell of sewer-gas was perceptible, and argues from this fact that the inodorous gases were the most dangerous ones. From an examination of the facts respecting Croydon the author concludes that there is no proof of the connection between the sewerage system and the outbreak of typhoid fever which took place there in 1875. He observes that he has devoted a large share of attention to this particular case, because it is the only one in which an epidemic of this nature has received careful scientific examination, and because it greatly supported the theory of sewer-gas infection. He states that this investigation forcibly recalls the report of Radcliffe, on the cholera epidemic in 1866, and his theory that the spread of the infection was caused by the mains of the East London Water Company, whereas Letheby most convincingly proved that the supply-pipes of the

Commercial Gas Company might with equal reason be suspected (*i.e.*, because both companies served the infected district), and that, as a curious coincidence, the first case of cholera occurred at the gasworks. Instances of outbreaks of an epidemic character are always more or less traceable to some one similar source of infection, and for this reason the water-supply, the milk, and such like, have been at various times accused. In a similar way Drs. Scott and Littlejohn attributed the fever-outbreak in Selkirk in 1876 to the bad drainage, and the Baden-Baden, Gibraltar, Caius College, and Dublin epidemics have all been set down to defects in the sewers. Dr. Soyka further refers to other diseases, such as erysipelas, bronchitis, and diarrhœa, which are said to have been propagated by sewer-gas.

Passing on to foreign experience, and selecting typhus as being essentially a disease whose spread is due to excrementitious matter and the emanations therefrom, the author gives careful tables of the health statistics of Hamburg, Dantzic, Frankfort-on-the-Main, and Munich, both before these towns were provided with a regular drainage system and after the drainage was completed; and shows by these figures that the death-rate from typhus has greatly decreased since the towns were thoroughly sewered. Taking another of the zymotic diseases, diphtheria, and considering the question whether or not it is gradually taking the place of typhus, he shows that the former is essentially communicated by direct contact, and that it is a disease infinitely more destructive in country districts than in towns, and one with which sewer-gases can therefore have but little to do. He also considers the prevalence of enteric diseases in the sewered and the unsewered portions of the same town, and shows that in every case proper drainage has largely diminished the mortality from these diseases. He gives the results of the investigations of Mayer respecting the cholera outbreak in Munich in 1873, and shows that the streets provided with sewers were much freer from illness and death than those which were undrained; the number of cases of illness being 230 per 10,000 in the undrained streets, and only 114 per 10,000 in those streets which were properly sewered. His conclusions are as follow:

1. It has been seen, in the first place, that the facts and arguments adduced in favor of the sewer-gas theory are by no means free from suspicion, and that, on the contrary, the demonstration is faulty and incomplete.

2. It has been proved that the sanitary conditions, more particularly as respects a special class of infectious diseases, have become substantially improved in towns provided with sewers.

3. That in towns in the various districts of which different methods or systems of excrement-removal prevail, the drained areas show no unfavorable prominence in regard to the presence of infectious diseases, and that, if indeed any connection is traceable between the sewers and such diseases, the influence of the drainage is a favorable one.

4. That the spread of certain infectious disorders (diptheria), which is believed to be dependent on the state of the town as respects the sewering, appears to depend upon entirely different conditions, and to put the whole matter briefly:

(1.) "The positive proof of a connection between sewer gases and the spread of epidemic diseases is wanting."

(2.) "The majority of the experiments hitherto made lead us to conclude that the spread of epidemic diseases is entirely independent of sewer gases, and that those towns, or parts of towns, provided with sewers are more favorably circumstanced, as evinced by their sanitary conditions, than the same towns before the drainage was commenced, or the districts which are still undrained."

PART II.—By DR. ALADAR V. ROZSAHEGYI, of Pesth.

The author states that at a time when vast drainage works are being undertaken, and so many important towns are adopting, or are prepared to adopt, the water-carriage system, it is advisable that the objections to this plan of excrement-removal, which have been raised on the score of the dangers arising from sewer gas, should be carefully and fully investigated. The theory that zymotic diseases are really due to the entry of sewer gas into dwellings is based upon the observation that the high-lying portions of towns, and those inhabited by the wealthier classes, which are then assumed

to be the higher-lying districts, are more liable to enteric diseases than the lower quarters of towns. The proofs brought forward in favor of this being that in certain affected houses the drainage was out of order, and that bad smells prevailed in the houses situated in the upper parts of towns. The reason alleged for this being, because, owing to its chemical composition, sewer gas is specifically lighter than atmospheric air, and naturally rises to these points; moreover, certain specific observations have been recorded in which a positive pressure was found to prevail in sewers. The inference from all these facts is, that sewer gas has a decided tendency to force itself outwards from the sewers, and consequently into houses.

From a consideration of the static and dynamic laws governing the movement of gases it may easily be argued that there are numerous factors which must be studied before any decision on this matter can be arrived at. Taking first the chemical nature of such gases, the author shows that the balance of evidence, excluding certain misleading experiments conducted with gases evolved from cesspools and closed vessels containing fœcal matters, leads to the belief that in lieu of being lighter than the atmosphere, sewer gases, owing to the presence of rather more than the usual amount of carbonic acid, are really heavier than air.

The differences in specific gravity of the sewer gas, due to the moisture it contains, are next dealt with, and the effects of the greater heat of the atmosphere in houses than in sewers, and in the sewers themselves than in the soil through which they pass, are noticed. The author shows that the flow of water in the sewer has in many cases an important bearing upon the air currents they contain. The state of the barometer also is not without a marked influence on the sewer gases, and the force of the wind has much to do with the pressure of the air in the sewers. He points out, finally, that the currents in different parts of the same system of sewers have in many cases a conflicting action upon one another.

The author states that he has dwelt at considerable length on these facts in order to prove that the gases in the sewers are exposed to numerous varying influences, thus rendering it very difficult to establish any general laws. He then details his own observations, which took place during the summer months, over a portion of the main sewers of Munich. He employed tobacco smoke to indicate the general direction of the air-currents, and sulphuretted hydrogen gas, with strips of paper dipped in acetate of lead and moistened with glycerine, to show the distances traversed and the time occupied by gases in passing through the sewers.

These experiments demonstrated the fact that the general direction of the air-currents in the main sewers was downwards, *i.e.* in the direction of the flow of the sewage water, and more markedly so in the deeper lying sewers, *i.e.*, those nearest the outfall. At the soil pipe openings into the houses the direction of the air-currents was very variable; more frequently, however, there was a draught into, rather than away from, the house. The ventilating power of the running water in the sewer appeared to the author so important that he carried out a series of experiments with tin pipes of elliptical section and fixed at various inclinations, having water flowing through them, both as a flat or as a deep pipe (⊂ or ○); and he gives a table of the air-velocities in these pipes under various conditions. His conclusions are as follow:

1. The air in sewers is influenced by a large number of factors, varying both as respects time and place, direction and force.

2. The results obtained during the summer months and when no rain fell were, that the sewer gases rarely passed upwards in the sewers, but, on the contrary almost invariably downwards; but that the more frequent tendency at the same time, of these gases was to stream outwards into dwellings.

3. House and street connections should be guarded against the entry of sewer gases, and means should be taken to dilute such gases freely with air.

4. The downdraught along the sewer in the direction of its fall is very favorable to this dilution with atmospheric-air and to the exclusion of the sewer gas from the lungs of the population, and every means should be taken to render the draught as powerful and as constant as possible.

AS TO THE DURABILITY OF BUILDING STONES.

From "The Builder."

WHILE fully aware of the general attention that has in all times been directed to the durability of stone, we yet question whether the subject has been anywhere exhaustively treated, either in our own country or on the Continent. Although holding closely to the need of experience, we yet should not forget that both chemical analysis and other methods of scientific investigation have made great strides of late, and that it may become essential to the architect to inquire how far they may throw light on the question of durability. We may practically know the difference in the durability of Bramley Fall and of Portland stone, but if we know not only the fact, but its cause, we have made a step in advance. This consideration will have more weight from some observations to which we shall have to refer as to the durability of granite.

For a contribution of much value to this investigation we are indebted to the Director-General of the Geological Surveys of the United Kingdom, Dr. Archibald Geikie, F.R.S. It is from the notebooks of geological rambles, and as regarded from the standpoint of the geologist, that the observations to which we have to refer have been extracted. None the less, it strikes us, do they form a very valuable beginning. Our own experience, and we doubt not that of many a reader, is enough at once to contribute some examples to those which have been elaborately investigated by Dr. Geikie; and we look forward with confidence to the preparation, sooner or later, of a comprehensive scientific work on the durability of building materials, in which chemical and lithological science shall have their due parts, side by side with the verdict of experience.

Dr. Geikie's researches have, in the first instance, been directed to the older burial-grounds in Edinburgh; the reason, of course, being, that as tombstones are usually date-bearing monuments, the means of comparing the progress of decay and the lapse of time are unusually precise. To these humble slabs we take leave to add, especially for the benefit of architectural travelers on the Continent, the category of scutcheons and armorial bearings. Many ancient buildings, especially in Italy, are adorned with stone armorial bearings. Of these the herald will be in many cases able to indicate the date with considerable accuracy. And, speaking now only from memory, we should say that a study of lithological degradation in Italy, based on dated works of this kind, will give results so widely different from those obtained by Dr. Geikie in Edinburgh as to point to the primary canon,—that the first division of any study of the subject must be topographical, or, rather, climatological.

Dr. Geikie points out that the effect of weather in a town is likely to be in some measure different from that which is normal in nature. The disengagement of sulphuric acid from the reek and smoke of chimneys is one of the causes of the more rapid decay of stonework in urban, as compared with rural, localities. On the other hand, the range of temperature is likely to be less active in a town. And the incrustation of the surface of the stone with dust, smoke and other inorganic as well as organic matter, in town buildings, has to be born in mind, although there may be a question as to the action of such incrustation on the interior substance of the stone.

Around Edinburgh the materials used are of three kinds,—1st, calcareous, including marbles and limestones; 2d, sandstones and flagstones; 3d, granites.

With few exceptions, the calcareous limestones in the Edinburgh churchyards are constructed of ordinary white saccharoid Italian marble. There may also be observed a pink Italian shell marble, and a finely fossiliferous limestone, containing *foraminifera* and fragments of shells.

The marble occasionally is employed as a monolith, in the shape of an urn, vase, or the like; but it has been usually fixed in a framework of sandstone. And it is as to its behavior in the latter case that the observations we have to mention will

prove to be novel to most of our readers. Dr. Geikie has, in the first instance, subjected specimens of the marble, both when freshly cut and when long exposed to the weather, to microscopic examination. His view of the process of degradation is that it is of a threefold character. The process of weathering, he says, in the case of this white marble, presents three phases, sometimes to be observed on the same slab, viz.: superficial solution, internal disintegration, and curvature with fracture.

With superficial solution we are tolerably familiar. It becomes apparent in the gradual dimness that comes over the polished surface of the marble. This is effected by erosion, partly by the carbonic acid, and partly by the sulphuric acid contained in the atmosphere, and notably in the rain that falls in towns. The rapidity of the process in Edinburgh depends very much upon aspect and exposure to rain. Exposure for not more than a year or two to the prevalent westerly rains is enough to remove the external polish, and to give the surface a rough character. The granules of pure calcite, which have been cut across or bruised in the cutting and polishing process, are first loosened or dissolved, and then drop out of the stone. An obelisk erected in 1864, in Grey Friars churchyard, is cited as an example in which the surface has already become so rough and granular that it might be taken for sandstone. The grains are so loosened that a slight movement of the finger will rub them off. The internal structure of the marble begins to reveal itself. The harder knots and nuclei of calcite project above the surrounding surface, and irregular channels, from which the lime has been carried away in solution by the rain, resemble the bleached and furrowed aspect of the rocks on the side of a mountain.

Solution, or decay of some kind, seems rather to be hidden than prevented by the formation of a surface-crust. This Dr. Geikie considers to form most rapidly where solution is most feeble in its apparent action. Beneath it the stone turns to a loose crumbling sand. In time the crust cracks into a polygonal network, and rises in blisters, exposing the under material to rapid disintegration. A marble urn erected in the same churchyard in the year 1792 is thus crumbling into sand, although it faces the east. The process, which Dr. Geikie describes with elaborate minutness, must closely resemble that which may be observed to take place with oolite stone in London; as, for example, on the south face of St. Paul's, where thick cakes of a black color may at times be seen to shell off, leaving partially disintegrated stone exposed to view.

It is the third form of decay, which Dr. Geikie describes as curvature and fracture, as to which, we think, the observations now recorded are the most novel. This most remarkable phase is to be observed in slabs of marble which have been firmly inserted into a solid framework of sandstone, and placed either in an erect or a horizontal position. It appears as a swelling up of the center of the slab, which forms, as it were, a blister that finally ruptures. A case is cited of a slab, 30 in. by 22 in., and ¾ in. thick, built into the south wall of Grey Friars churchyard. The date of the last inscription on it is 1838, at which time it is presumed that the slab was smooth and upright. It has now escaped from its fastenings on either side, though still held firmly at top and bottom, and projects from the work like a well-filled sail, to the distance of 2½ in. A series of rents, one of which is one-tenth of an inch in width, has appeared along the crest of the fold. In another case, that of a tomb erected in 1799, facing south, and protected by overhanging masonry from the weather, the inscription has become partly illegible, the stone has bulged out in the center, and cracks begin to riddle the blister. On another slab, twenty years older, dated in 1779, on the west wall, the process of destruction has advanced to a further stage, and since it was sketched by the author of these notes, has altogether fallen out and disappeared.

It is the opinion of Dr. Geikie that this mode of destruction is due to the action of frost. As to this we are disposed fully to agree with him, and that from observations of our own which bear on the subject. One set of these regard the durability of marble where frost is unknown, or rare. For example, we can cite a large marble tablet built into the wall by the eastern gate of the little

archiepiscopal city of Sorrento, which contains (or did some years ago) a long and perfectly legible Latin inscription, of the date of the Spanish rule in Naples. Again, on the gates of the City of Naples, and on the Castel Nuovo in that city, are scutcheons of arms which have been defaced on some occasion of change of dynasty, and on which the marks of the chisel are so fresh that it is clear that the absence of armorial bearings is not due to the lapse of time, but to political causes, and purposed violence. In those instances, to which a very moderate acquaintance with Southern Europe can no doubt add many more, we have ample proof of the monmental durability of marble, although freely exposed, in a climate where frost is very rare, and never of sufficient intensity to get good hold of the surface of the ground. The other observations refer to the curious permeability of limestone to wet. It may be said, perhaps, that the water which collects on the interior surface of a limestone or marble wall does not percolate, but is condensed by the cold of the wall from the atmosphere. Weeping through solid stone seems, indeed, incredible. But we can cite one instance of a wall made of mountain limestone, thoroughly well built, and 3 ft. thick, in H.M. Dockyard, Pembroke. It is the wall of a smithy. When it was newly built, when the rain drifted on it from the west the wet ran down within the building as if the walls had been of chalk, or some porous substance. We do not assert that the wet did come through the walls. But it appeared so to do. And, at all events, this and other experiences point to a hygrometric condition in the purest and densest limestones which is likely to have a very destructive effect in the event of the occurrence of frost directly after rain.

Dr. Geikie comes to the conclusion that the lowering of the surface of marble by superficial solution may amount to $\frac{1}{3}$ in. in a century; a reduction to a pulverulent condition in about forty years; and a total disruption by curvature and fracture in a century. We only add the condition that this must be where frost is energetic in its action.

The endurance of sand-stones and flagstones is a question of selection. In those which consist almost wholly of silica, the durability is very great. Some of these stones contain as much as 98 per cent. of silica. A tomb of this material is cited which was erected in 1646, and ordered by the Scottish Parliament to be defaced in 1662. The original chisel-marks are still fresh on the surface of the stone (as in the case of the scutcheons at Naples), on which the lapse of 200 years has produced little effect, except that of somewhat roughening the exposed faces on the west and north sides.

In cases, however, of striated or colored sandstones, destruction goes on by solution of the cement or matrix in which the particles of silica are embedded. The most common kinds of matrix are clay, carbonates of lime and of iron, and the hydrous and anhydrous peroxides of iron. In one case of a stone of this kind an inscription, cut in 1863, is no longer legible. We should like to know the depth to which the letters were originally cut; $\frac{1}{8}$ in. at least has been removed from the stone in sixteen years, which is at the rate of nearly $\frac{3}{4}$ in. in a century.

The well-known propriety of the rule for setting stone on its natural bed is illustrated by the degradation of laminated flagstones when set on edge. Dr. Geikie cites an instance in the case of stones thus treated of the loss of $\frac{1}{3}$ in. in thickness in forty years, which rather exceeds $\frac{3}{4}$ in. in a century. A curious instance is also given of pillars of a concretionary sandstone, which exposure to the air for 150 years has hollowed out into positive troughs, with hollows from 4 in. to 6 in. deep, and from 6 in. to 8 in. broad.

As to granite, we are referred to the experiments of Professor Pfaff, of Erlingen, described in the *Allgemeine Geologie als exacte Wissenschaft*, p. 317, on granite, syenite, Solenhofen limestone, and bone. From the limestone the Professor found the loss to amount to the removal of a uniform layer of 0.04 millimeter in three years, which gives 0.52 in. in a century. The annual loss of granite he estimated as 0.0076 millimeter per year from unpolished, and 0.0005 millimeter per year from polished surface. This difference of more than 10 per cent. against the latter is contrary to what would have been expected; and it has to be asked for what period of time the more rapid

weathering is supposed to continue. The slower rate amounts to 0.30 in. per century. Granite has been employed monumentally in Edinburgh for too short a time to allow of the measurement of its rate of decay there. But in connection with the subject we may be allowed to recall remarks made in the columns of the *Builder* nearly twenty years ago on the subject of the rough and granulated surface of the granite on the west face of Waterloo Bridge. The arches and exterior face of that bridge are built of Cornish granite, from the vicinity of Penryn, and the balustrade is made of fine grey Aberdeen granite. A careful and exact admeasurement of the projections of this bridge, compared with the original dimensions, would enable the student to arrive at a correct estimate of the rate of weathering of these two kinds of granite in London. The bridge was opened in June, 1817.

The close of this interesting specimen of the "Geological Sketches" of Dr. Geikie refers to the fact that in the towns and villages in the north-east of Scotland, where the population is sparse, and where comparatively little smoke passes into the air, the marble tablets last longer than they do in Edinburgh, but still show everywhere indications of decay. They suffer chiefly from superficial erosion, but cases may be observed of curvature and fracture.

In contrast to the perishable character here ascribed to granite, to marble, and to any but the purest silicious sandstone, is the durability of the humble material, clay slate. This is employed for monumental purposes in Aberdeenshire. It contains cubes of pyrites, which might have been anticipated to prove sources of destructive chemical action, but which seem to be inert. The stone is easily dressed in thin smooth slabs. A tombstone of this material erected in the old burying-ground at Peterhead, between 1785 and 1790, retains its lettering as sharp and smooth as if only recently incised. The stone is soft enough to be easily cut with a knife. The cubes of pyrites are covered with a thin film of brown hydrous peroxide. The slate is slightly stained yellow round each cube, but its general smooth surface is not affected. While neighboring marble tablets, 100 to 150 years old, present rough granular surfaces and half-effaced inscriptions, the lapse of nearly a century has produced scarcely any appreciable change upon the clay slate.

The durability of this material, when prepared by nature for the stone cutter, may be compared with that of the even humbler, but equally durable substance of artificially baked clay. In the dry and frostless air of Egypt, marble and granite are almost perennial in their duration. But the main revelation of the forgotten history of the past is derived from the baked clay inscriptions of Assyria. The inertness of this substance, its hygrometric resistance, and feeble chemical affinity with any element with which it comes in contact, is the cause of its indifference to the passage of time, or rather to the recurrence of those changes of temperature and of moisture which accompany the revolution of the year. If the value of clay slate, as a material for monumental inscriptions, had been better and larger known, how much would our churches and churchyards tell, which is now wholly unrecorded?

The chief cause of the interest which we took, from the first hint of this publication that reached us by chance, in these researches of Dr. Geikie, was the hope that they would throw some definite light on what we regard as the most difficult, and one of the most interesting questions relating to any monuments in Europe, viz., the age of Avebury and of Stonehenge. Nor are the remarks without direct bearing on that subject. The stone known as "Sarsen" fulfils the requirements above shown to be conducive to the most permanent durability. It is compact, uniform, close-grained silex. We cannot cite any chemical analysis of the stone. But we do know that the Wiltshire farmers have found it so indestructible by the usual instruments of agricultural violence, that they had recourse to the barbarous plan of roasting these priceless monoliths, heaping faggots on them to make a bonfire, and then throwing on cold water to crack the stones! This argues wonderful resisting power in the "Sarsen," and no one can be familiar with the stone in question without seeing that it affords the least possible advantage to the tooth

of time. Time, indeed, as Dr. Geikie observes, is not an agent, except indirectly, in the matter. Mere duration from day to day has little or nothing in it that is destructive, as we see in Egypt. It is because the revolution of the year, and the succession of the seasons, expose a monument to the successive and ever-repeated attacks of rain, of frost, of perhaps the scoring draughts of well-driven sand, and because the incessant repetition of these small causes of decay produces a great accumulated effect, that we regard times as destructive. But too much attention cannot be given to the consideration that it is the action of severe frost on stone containing water that is the main cause of decay. And we venture to suggest, as a subject for careful chemical analysis, how far the existence of water, or the elements of water, not as moisture, but as chemically combined with lime, magnesia, or other elements, in a stone, may render it susceptible to the attacks of frost. That idea is, perhaps, a new one; but we feel certain that the hygrometric relations of marble and compact limestones are not by any means clearly understood. The effect of frost on these stones has been shown. This view of the case makes it the more necessary to repeat and to comprehend the experiments of Professor Pfaff on granite. In anticipation, any one would have said that polished granite would be the most durable; and the idea that it would most thoroughly throw off the rain, and thus escape soaking and subsequent frost splitting, concurs with this antici pation. If the case really prove to be the reverse, we can see no explanation for it, except in the possibility of the bruising of individual molecules of feldspar in the process of polishing, so as to make them more readily absorbent.

But this is a subject that will repay the most careful experiment.

As to the Wiltshire monoliths, we think that the whole inquiry above mentioned points in the direction of their immense antiquity. The only chance, so to speak, of Time for attacking them is when they are so set as to expose the ends of what really is, though not visibly, the bed-course. Those who know Avebury will remember themarks of decay on some of the 18-ft. monoliths that form the sides and roofs of the cellæ. The inference, seen from the light of the Edinburgh observations, points to enormous age. Let us add that, at a distance from the spot, we have no means of determining the chemical constitution of the "blue stones" in the inner ring of Stonehenge, or their present condition as compared to that of their giant brethren in the trilithons. Here is a subject for careful observation, analysis and record. And it may prove that a comparison of the chemical constitution and lithological condition of these two kinds of stone may enable the man of science to construct something of an archæological calculus that will throw light on the date of Stonehenge.

REPORTS OF ENGINEERING SOCIETIES.

AMERICAN SOCIETY OF CIVIL ENGINEERS.—The latest issue of the Transactions contains:

Paper No. 242.—On the Overflow of the Mississippi River. By Lyman Bridges.

Paper No. 243.—Highway Bridges. By James Owen.

At a meeting of the Society held Wednesday, Sept. 20th, a paper describing the methods used in a rapid topographical survey of a portion of the Gold Field of Nova Scotia, by Wm. Bell Dawson, was read in the absence of the author by the Secretary. This survey was made by the use of stadia hairs and a Rochon micrometer telescope for the measurement of distances and resulted successfully and with very moderate expense. Col. Wm. H Paine, Vice President of the Society, described the methods in use by him in making surveys for the campaign maps of the Army of the Potomac during the war, observations often being taken from the tops of trees and the resulting maps showing remarkable accuracy. Mr. Robert B. Stanton, M. Am. Soc. C. E. of the U. P. R.R., also described rapid surveys made by Mr. Blickensderfer and himself in preliminary reconnoisances for the Pacific Railways.

ENGINEERING NOTES.

ENGINEERING STRUCTURES IN ITALY.—A paper was prepared lately by Signor C. Clericetti on the "Great Structures erected in Italy during the last Twenty Years."

The author chooses the bridges of iron and stone erected during the last twenty years as the structures which best exhibit the progress of engineering science, and he compares these modern bridges with those built by the Romans. The characteristics of these latter are grandeur, massiveness, and durabilty; of the former, lightness, economy, and rapidity of construction.

The Po between Pavia and the sea was never bridged by the Romans, but during the last twenty years four bridges have been built over it. The lengths of these bridges are 577, 762,

427, and 400 meters; 1,900, 2,600, 1,399, and 1,312 feet respectively; the spans varying from 213 to 250 feet. They are all girder bridges, supported on piers founded at depths of from 60 to 70 feet below highest flood level, and formed of iron cylinders sunk by hydraulic process.

To show the difference between the ancient and modern systems of construction the author compares the Roman bridge across the Danube, one of the boldest of their works, with the modern structures on the Po. The former—1,207 meters (3,960 feet) in length—had twenty-one wooden arches of 50 meters (164 feet) span; and the piers—founded on a masonry platform extending right across the river bed—had a thickness of 17.7 meters; while the piers of the latter, though 28 meters high from the foundation, are less than 3 meters thick at the top. The ancient piers had six times the thickness required for a modern girder bridge, and three times what would now be allowed for masonry arches of 50 meters span. The same immense piers were built throughout the middle ages; the old bridge at Verona, for instance, with two arches of 21.54 meters and 48.70 meters (93½ and 160 feet), has a pier 12 meters thick, though only 3.50 meters high.

The author proceeds to point out the superiority of the modern system of long spans and narrow piers, in leaving the channel free for navigation and the discharge of floods, and avoiding the scouring action caused by obstacles to the natural flow. In some cases old bridges have so impeded the flow as to cause serious inundations above bridge.

The author states that, with few exceptions, only one type of bridge—the lattice-girder—is constructed in Italy, and regrets that little encouragement is given to improvements in design. He mentions a few arched bridges, among them being that over the Celina torrent, which he considers one of the best examples.

The author proceeds to discuss the subject of the incalculable strains to which bridges are liable; from the points of support not being knife edges, as theory supposes; from the vibrations in cross sections; from the vibration caused by passing trains, &c. Airy attempted to ascertain the strain in a bar of iron from its musical note, but the result was not satisfactory. Better results are obtained by instruments for measuring the contraction and elongation of bars during strains, such as the apparatus of Dupuit and Manet in France, and Castigliano's multiple micrometer, which the author describes.

The experiments made with Dupuit's apparatus upon all kinds of girders show that the actual maximum strains are in general less than the calculated, particularly in arches and in the horizontal members of straight girders. Iron bridges are also exposed to danger from corrosion, but the author states that Mallet's experiments proved that an iron bar 6 millimeters (0.238 inch) in thickness would not be destroyed in less than 700 years.

The author then gives particulars of some of the principal brick and stone bridges recently erected. Comparing modern with ancient structures, he points out that the former are built with one-third less material than the latter. In ancient structures the ratio between the thickness of the piers and the span varied from one-fourth to one-half, while in modern it has been reduced to one-sixth, and even one-seventh. The average ratio between the thickness of the arch at the crown and the span was 0.086, while in modern bridges it is from 0.040 to 0.031.

The two principal arched bridges erected in Italy during the last few years are the Ponte Annibale and the Ponte del Diavolo. Each of them has a span of 55 meters (180 feet), and thickness at the crown of 2 meters, the versed sine of the former being 14 meters, of the latter 13.55 meters. Circular openings 9.25 meters in diameter are introduced to lighten the haunches. These are the largest masonry arches in the world, with the exception of one at Chester of 61 meters span, and one on the Washington Aqueduct in America of 67 meters. In the year 1370, however, an arch of 72.25 meters (237 feet) span, and 20.70 meters rise, was erected over the Adda, at the Castle of Trezzo. This arch was considered the eighth wonder of the world, both for size and for the short space of time—seven years and three months—occupied in its construction. The Ponte Annibale and the Ponte del Diavolo were built in twelve and ten months respectively. Among recent improvements in detail the author mentions the use of hydraulic lime and cement, which allows the centers to be struck very shortly after the completion of the arch; and the use of sand-boxes instead of wedges for slacking the centers, a system which he strongly recommends.—*Architect.*

THE CHANNEL TUNNEL.—At the meeting of the Paris Academy of June 26, M. Daubrée read a note on the geological conditions of the Channel tunnel. The works connected with the tunnel comprise three phases:—(1) Scientific researches; (2) preparatory works; (3) execution of the tunnel itself. The first phase was devoted to purely geological investigation, in the form of minute exploration of the French and English coasts, exact and detailed investigation, of the sea-bottom in the Strait, borings made on *terra firma* which verified the nature, thickness, and inclination of the strata, and gave an approximate idea of the hydrological condition. Since 1879 the second phase has been entered on by verifying the previous scientific data, and preparing for the execution of the tunnel itself, experimenting in small galleries with machines and tools capable of being ultimately used in a work of exceptional importance. On the French coast, the geological investigation established a slight bulging of the beds at the place known as the *Quenocs*. On account of this bulging the inclination of the strata, which, in the strait is towards the north-north-east, is found, along the cliffs of Blanc Nez, turned towards the south-east, and the slope which, according to the first orientation, in the neighborhood of the Quenocs, is about 0.05 per meter, is found, in the second, to be nearly 0.09 m. It is important then, to find in what conditions this bulging may modify the physical conditions of

the banks forming the base of the Rouen chalk. For this purpose the French Association had dug, near Sangatte, two shafts of a depth of 86 m., which met the gault at 59 m. below the hydrographic zero, adopted in the maps in which the geological explorations of 1875–6 are recorded. The digging of these shafts, one of them 5.40 m. in diameter, showed that all the white chalk and the upper part of the Rouen chalk are water-bearing. These strata had thus to be abandoned

On the other hand, the base of the Rouen chalf allowed only a very small portion of water to pass. There, then, the tunnel should be pierced, as the stratum appeared to proceed without interruption from France to England. The water penetrating the works is fresh, and of good quality; at the upper part only some slightly salt veins were found. Nevertheless, the communication of the water-bearing strata with the sea is proved by the oscillation of the water-level in the shafts according to the tide, and by the invariable increase at high water. M. Daubrée then refers to further galleries dug on the French and on the English sides, and excavations made with the machines of Col. Beaumont and Mr. Brunton. On the Dover side, the chalk, which on the French side was but little permeable, was, on the English side, quite impermeable. Owing to this circumstance, they were able to begin at the bottom of the shafts, at 29 m. below the French hydrographic zero, a gallery advancing under the sea by following in the stratum an almost regular descending slope of one-eightieth, or 12.5 mm. per meter. The bed on the English side, somewhat more powerful than on the French side, presents a very great regularity. Thus the Beaumont machine, which has been used in the perforation, has been easily able to trace a perfectly cylindrical gallery, which has now reached 1,800 meters from the shafts, of which 1,400 meters are under the sea. So far there has been no access of water. In the banks which form the base of the Rouen chalk, the rock in mass is almost completely dry; the access of water which has been observed has entirely the character of small springs issuing from the joints of fracture or cleavage. The perfectly cylindrical form produced by the Beaumont machine renders the gallery where such leakage occurs easily isolated by means of cast-iron rings prepared in segments easily united, the rings themselves being clamped together to form a tube of any length. When the water spurts out in considerable force, a sort of mastic or minium is successfully employed, which is placed between the segments of the rock, and compressed in the manner of a water-joint by the pressure of the rings against the rock. The mastic also seems to render the joints of the neighboring rings water-tight. Owing to the excellent make of these rings, they can be rapidly put in position; a complete ring can be placed in half an hour, and several experiments in the Shakespeare Cliff Gallery have proved that by this simple process the springs encountered can be completely blocked. On account of the slope on which the English gallery descends, its extremity recently reached 51 m. below the hydrographic zero, at a point where the depth of the sea at low water is 5 m.; there is thus 46 m. of chalk between the floor of the gallery and the bottom of the sea.

PROPOSED TUNNEL UNDER THE ELBE.—Under the river Elbe, at Hamburg, it has been proposed to build a tunnel to connect that city with an island a third of a mile distant. The great Hanseatic city, which has hitherto been a free port, is shortly to lose that privilege, and to be included in the Zollverein or German Customs Union. It is intended, however, to make an exception in favor of the island in question, which bears the name of Steinwarder, and to permit it to retain the privileges of the free port. Large bonded warehouses will be built there for the accommodation of merchandise before paying duty, and in order to bring the island into closer connection with the city, the above-mentioned scheme for a tunnel under the river has been started. The tunnel would be 500 meters or nearly a third of a mile in length. This will be upwards of 300 feet longer than the Thames Tunnel. The cost of the Elbe Tunnel is estimated at about £900,000.

THE LARGEST LOCK IN THE WORLD.—It will be of interest to all those who either support or oppose the scheme for a ship canal to Manchester to know what is, at present, the largest lock in the world. In a statement recently submitted to the Congress of the United States this is said to be on the St. Mary's Falls Canal. "The canal is slightly over one mile in length. There are two locks to overcome the same elevation, one being the largest in the world. It is 515 ft. in length, 80 ft. wide, and 18 ft. lift." The estimated yearly expense of working it is $25,000. On the Lousville and Portland Canal, which is 2.15 miles long, are two locks 372 ft. long and 80 ft. wide, with 12 ft. and 14 ft. lifts. These locks were worked by hand in 1879; 3,168 vessels of all classes passed through the canal in that year. A tow-boat, a dock, and steam dredges are maintained. The expenses for 1879 were $30,928, of which $14,453 were for dredging. The North Sea Canal is stated in the same report to be sixteen miles in length, and from 130 ft. to 400 ft. in width. The level is below that of the sea. There are two sets of locks of large dimensions, and an artificial harbor constructed under great difficulties. The depth, originally 23 ft., is to be increased to 26 ft. by 1884. The cost of the work was $10,800,000. From November, 1877, to August, 1879, 4,862 vesssels passed through the canal. The working expenses for the only year for which they have been obtained were $75,569. There are eight miles of canal to each lock-lift. On the Des Moines Canal, 7.6 miles long, there are three locks, suitable for the longest steamers on that river. The annual expenses are above $30,000, including a large amount for dredging. A detailed estimate of the number of minutes occupied in each of the eight operations involved in the process of going through one of these large locks amounts to 20½ minutes. At St. Mary's Falls the approaches are not completed, and cause material

delays, yet lockages do not occupy half an hour each. The reporter concludes thus:—Probably, in almost every location where water is to be had, a better ship-canal can be made with a few locks, and at a far less cost than the sea-level canal. A small part of the money saved by the locks will, in most cases, make a broad and deep canal, where ships can go safely and rapidly, and pass each other anywhere without delay; instead of narrow deep cuts, commonly dangerous and always expensive, where ships must move slowly, and wait to pass each other. The question must be decided in each case whether the large amount required for the construction of a lock will save a larger amount some other way, and whether the delay at each lock will save a greater delay in some other way.

RAILWAY NOTES.

A CHEAP RAILWAY.—There is now at work an interesting miniature railway—five miles in length—which unites the village of Westerstede in East Frisia with the station of Ocholt, on the Oldenburg and Seer line. It is solely due to the enterprise of the thinly-scattered population of the district, and carries their cattle and other produce to market, bringing them back their few requirements. The soil is marshy, so that a good deal of drainage work had to be done, and it was necessary to carry the line above the level of the frequent floods. In spite of this, the cost of construction was only £2103 7s. 6d. per mile; and the cost of working (including wages, fuel and every expense) amounts to the magnificent total of £1 7s. 6d. per diem. The buildings consist of a shed at each end of the line; the terminus is the courtyard of the principal inn at Westerstede, and the single station—half way along the line—is the house of a gentleman, who hospitably entertains the passengers while they are waiting for the train. The rolling stock comprises two small four-wheeled tank locomotives, weighing (when in working order) 7½ tons each; three carriages, of the American type with a door at each end; two open goods trucks and two covered. A train consists of the engine and two vehicles, between which the guard sits. There are no turn-tables, so that the locomotive is at the hinder end of the train in returning. The fuel employed is turf, which is abundant in the district. The receipts of this tiny railway are steadily increasing.—*Engineering.*

AT a recent meeting of the American Master Car Builders' Association the President suggested for discussion: "Is it safe to run a journal under passenger trains after it has been heated sufficiently to burn out the packing and cooled off with water?" Mr. Bissell said: It is usually the case that new cars running out of the shop will run warm if ever. Sometimes it will be so warm as to discolor the paint on the box and spoil it. I think it is very seldom the case that they take those journals out that heat up. The President said: Carbuilders, as a rule, pack their boxes very shabbily, and they almost always get hot; but they are very seldom allowed to get hot enough to burn the packing out and to be cooled off with water. I have taken great interest in trying to learn what was the cause of journals breaking off at the shoulder, showing no fracture, while the center of the axle would show a remarkably good quality of iron. A few days since I was testing some axles, and during the test I put under a few old axles, and at the second blow on one of them the journal flew off into the air I should say ten feet or fifteen feet, simply with the jar of weight dropping upon the axle. The axle was tested with a 1600-lb. drop, and, in order to find out the quality of iron in the axle, I concluded to break it, and if my memory serves me, I would drop that 1600-lb. weight fifteen feet, reversing the axle each time seven times before we broke the axle. Now the journal showed no fracture of any description. It was completely crystallized, and I am very strongly of opinion that that was caused by meeting, in the first place, a cooling off with water under load, and I am so thoroughly satisfied on that point that my instructions are to remove every axle that has been heated sufficiently hot to be cooled off with water. I have seen several instances where the journal dropped off and was found in the box and the car came in safely. In fact one or two of the Pullman cars have come in with the journal lying in the oil-box. While I don't doubt that the axles were of good material, I firmly believe that an axle, after it has been heated sufficiently hot to burn the packing out and cooled off under load, is an unsafe axle; and by microscopic examination of the journals that drop off in that way, you will observe that there is a yoke very often the whole distance round the axle at the shoulder, showing that under load the journal bent as it revolved.—*Engineer.*

STEAM TRAMWAYS IN LONDON.—The London Street Tramways Bill, notwithstanding considerable opposition, has passed through committee in the House of Lords, and thus the thin edge of the wedge for the introduction of steam as a moving power for tramways in London has been successfully inserted. The bill provides for the construction of a tramway along the Pentonville Road from the Angel, Islington, to King's Cross. Pentonville Road having a very steep gradient, the cars will be driven by stationary engines placed at several points on the line, on a principle already in use in America, that is to say, by wire ropes passing under the permanent way. We are sorry to see this, although the tramway itself will be of very great convenience, completing the link that was much wanted between the Great Northern and Midland Railways and the tramways which branch from the Angel, Islington, to the north and east. But the nuisance which will arise from stationary engines to the neighborhood will be intolerable. Lords, and, for that matter, Commons, however, do not reside in the North or East of London. We are sure they would never permit the introduction of steam tramways in the fashionable quarters of the West.—*Iron.*

THE ELECTRIC RAILWAY IN IRELAND.—The works in connection with the electric tramway between Portrush and Bushmills, County Antrim, says the Glasgow *Herald*, are now approaching completion, and it is expected that the line will be formally opened for passenger, goods, and general traffic by the Lord Lieutenant early in August. Electric tramways have been already worked successfully in Berlin and in Paris, but to County Antrim will belong the honor of having introduced the new motive power for the propulsion of carriages and wagons within the United Kingdom, and 50 years hence the six miles of railway leading to the Giant's Causeway may share the historic interest of the line between Stockton and Darlington. The new scheme is to be considered from two points of view—the scientific and the financial. Viewed from the former standpoint, the new tramway presents several novel features of construction. The line, instead of being laid along the center of the roadway, is placed upon the side of the road, on a "trampath," from which the ordinary road traffic is excluded, but which suits as a footpath when so required. At the Portrush terminus is a building for the engine and dynamo-machine which develop the electricity, the patent adopted being that of Siemen. The rails are made of the best steel, and, no heavy engines being required, will be subjected to comparatively little wear and tear. The cars are also of the lightest construction, and friction will be reduced to a minimum. The project, looked at from a financial point of view, gives every prospect of success. The tramway will communicate both with the quays and with the railways at Portrush, and, besides the passenger tariff, several sources of revenue are already assured, including the carriage of goods and animals, iron ore and limestone. In addition to the indirect gain resulting from diminished deterioration of rolling stock and permanent way through decrease in friction, it is estimated that the cost of working the new line will amount to only one penny per mile as compared with seven pence per mile for steam-power, and eleven pence for horse-power. One large item of profit arises from there being no need of engine-drivers and stokers, the conductor being able, unaided, to regulate the movements of his car. Finally, the cost of construction has been greatly kept down from the company having themselves carried out all the works in connection with the line.—*Engineering News.*

IRON AND STEEL NOTES.

ART CASTINGS IN IRON.—A new departure of great interest has recently taken place in iron founding. This is the reproduction of various art works in iron castings. Shields ornamented with repousse work, helmets ornamented in relief, medallions, plaques, and Japanese bronze trays have been used as patterns, and successfully copied.

The work has been done in an iron foundry in Chelsea, Mass. The most delicate patterns have been successfully followed. One large shield represents the siege of Troy, and is a copy of Cellini's shield. The numerous small figures are brought out clearly, and defined with precision. The shield is 22 in. by 28 in., and is colored to represent bronze. This bronzing is produced by copper deposited by electricity. Another shield, heart-shaped, and 22 in. by 26 in., depicts the conflicts between Jupiter and the Titans. This has the natural color of the iron. Two circular shields show Bacchus armed with the thrysis and accompanied by a leopard. A triumphal procession is represented on a large salver. A copy of a bronze plaque with a head of Shakespeare and a reproduction of some repousse work after Teniers are also to be seen.

A helmet elaborately ornamented with intricate designs has been reproduced from a casting made at the Ilsenburg foundries, in Prussia. Many fine castings have been made there, but there has been no attempt at classical art in the designs employed. Some antique swords with curious hilts accompany the helmet. Even more interesting are the reproductions in iron of two medallions. One is a profile portrait of F. D. Millet, by Augustus St. Gaudens, and the other is the portrait of a young lady. In both the iron is bronzed. There are two small panels in iron, which have been "buffed" until they look like steel. One bears an exquisite chrysanthemum with its delicate grace preserved in the prosaic medium in which it finds expression. The other bears some leopards taken from antique bronzes.

A Japanese lacquer tray, with fine ornamentation, has also been reproduced in iron only a sixteenth of an inch thick. A medallion, with a head of Apollo in alto relief, is as striking as the foliage and flowers that have been executed in low relief. The bronze castings resemble beaten work in copper.

There are no especial peculiarities about the production of these castings. American iron is used, the moulds are of fine sand, and the best workmen and the greatest care are employed. The "facing" of the moulds is of dust from the beams of the foundry. Impressions are secured in the sand of the shield or panel to be cast, and the mould formed in the usual way. The casts are put under a rag-wheel with emery to prepare them for plating. The work has been treated in different ways, being polished to show the color of the metal, bronzed, copper-plated, and oxidized, simply that varying effects might be studied. The experiments have proved that remarkable firmness can be obtained successfully in work in iron, and the art castings will now be placed on a commercial basis.

The first work done in this direction was by the same company in 1876, when plates were cast from compression bronze patterns. About two years ago the matter of art casting was taken up, in connection with an attempt to introduce artistic work into the ornamentation of stoves. One advance led to another, until in the course of time the production of these art castings followed.

The attention of architects and interior decorators has been attracted already. For plaques to be hung upon the walls these reproductions

are rather heavy. But a ready use is expected for iron panels, reproducing repousse or other ornamental work, to be used in doors, in furniture, on the fronts of the steps, in stairways, or in fireplace linings. Original patterns, of course, can be employed. Panels may also be used in friezes and dados and in a great variety of decorative forms. A more directly architectural use of artistic iron castings is in balustrades and railings. Compared with bronze work, beaten by hand, the cost of these iron castings is very slight. An estimate was made that the reproduction of an elaborate bronze salver, with repousse work, in bronzed iron could be sold at a profit for ten cents a pound.

THE INFLUENCE OF MANGANESE ON THE STRENGTH OF IRON.—By Dr. H. WEDDING, Prof. FINKENER, and Prof. SPANGENBERG.—A prize of £100 having been offered by the Society for the Encouragement of Industry in Prussia for the best series of alloys of iron and manganese, two manufacturers submitted samples, the examination of which is detailed in this paper. According to the conditions of the competition twenty rods of iron were to be sent in, ten of an alloy of iron and manganese with less than 0.6 per cent. carbon, and not more than 0.4 per cent. impurities; and ten of an alloy rich in carbon, and in which the impurities were not to exceed 0 6 per cent. The proportion of manganese in the first series of samples was to increase gradually from 0.5 to 5 per cent., while the amount of carbon in the second series was to vary by increments of at least 0.15 per cent. The rods or bars were to be perfectly homogeneous, and 50 centimeters (19.685 inches) long by 40 millimeters (1.575 inch) thick.

The chemical examination, which included a careful analysis of each bar, was carried out by Professor R. Finkener, while the mechanical tests were entrusted to Professor Spangenberg. The analyses of the first ten bars showed that the proportion of manganese varied from 0.42 to 0.88 per cent., while that of the carbon was from 0.36 to 1.94 per cent. The second series of ten alloys by the same maker were found to contain carbon in proportions varying from 0.29 to 0.74 per cent., instead of the stipulated minimum of 0.6 per cent. The percentage of manganese rose from 0.24 to 4.37. The first series of samples submitted by another firm contained: manganese, 0.32 to 11.4 per cent.; carbon, 0.58 to 2.42 per cent.; maximum impurities, 0.92 per cent. The second series showed a gradual increase of manganese from 0.35 to 2.21 per cent., the amount of carbon rising at the same time from 0.58 to 2.9 per cent. From these analyses, which are given in detail, it appeared that none of the competing series completely fulfilled the prescribed conditions with regard to chemical composition. It was found in carrying out the physical experiments with these alloys that they were extremely hard, and so brittle that they frequently flew into numerous pieces when subjected to a transverse strain. The tensile strength did not appear to bear any fixed relation to the amount of carbon or manganese present, and in many cases the alloy was not homogeneous. The impurities, especially the phosphorus, contained in the samples tested may have had more influence on the mechanical results than either carbon or manganese.—*Abstract of Inst. of Civ. Eng.*

ORDNANCE AND NAVAL.

THE MONCRIEFF SYSTEM OF PROTECTED BARBETTE.—Colonel Moncrieff has addressed the following to the *Times* on this subject: All the reports of the Moncrieff battery at Alexandria that I have seen go to confirm the opinions generally entertained regarding the system which it represents. I do not know how far my principle was complied with in the profile of this particular battery; while, however, the other batteries were reduced to ruins, their guns dismounted, and the men blown to atoms by the terrible artillery fire to which they were exposed, the solitary Moncrieff battery, although receiving a full, if not a greater share of the attack, remained a perfect shelter for the men working it, and was fit for action to the last. I trust that this result will lead to the further development and application of my system. The English authorities, through my agency, have in recent years developed the system thoroughly for siege artillery, with the best results; and, at the recommendation of the committee which was entrusted with the experiments, Moncrieff siege carriages have been adopted in the service, as well as those for permanent works, and it is to be hoped that an opportunity will also be afforded to test their advantages in the field. But the authorities have declined many applications from me to be allowed to use the system for 18-ton and heavier guns for coast defence, it is thus restricted to land service guns up to the weight of 12 tons. Its value for coast batteries is thereby almost lost. It is my opinion that the system which has worked so well with the siege carriages is better suited for 18-ton and heavier guns than for the lighter guns to which it is actually applied. This opinion has been frequently expressed, and many designs and proposals submitted for carrying it out. I would desire to direct the attention of the service to the long delay in applying the system to land service guns above 12 tons, in the hope that opinions may be formed and expressed at this time which may induce the authorities to resume the application of the system for heavier artillery, for which everything is ready except permission to begin. When the time arrives for using our defences, I am certain that it will be regretted that this system is not applied in those positions inwhich it is admitted to be the best that can be used, and that the recommendations of the numerous committees which have recommended its application to them on the double grounds of economy and efficiency have not been carried out. It may now be said that the reports of these committees are predictions of what has actually happened at Alexandria. It is now some time since I exhausted all my means of pressing this

matter. I trust that others, on public grounds, may now come to my aid in urging the importance of the subject, and in having the system applied to heavier guns on our coast defences. Recent events have proved it to be able to resist naval attack, and it only requires to be applied to heavier artillery to make it available in many positions which would at once become much more formidable by its application.

COMPOUND ARMOR-PLATE TRIALS.—Further experiments at Portsmouth confirm in a marked manner, says the *Time*, the extraordinary results previously obtained from compound (steel-faced) armor. The admiralty having increased the severity of their tests on board the *Nettle* by the introduction of a 10-inch gun, one of Sir John Brown & Co.'s Collingwood armor-plates, manufactured on the Ellis principle, was fired at on July 11. Having in the meantime been removed from the target, it was examined recently for the purpose of ascertaining the effects of the ordeal upon the iron backing. The dimensions were 7 feet 9 inches by 5 feet 10½ inches by 11 inches. The plate had been previously fired at with the 9-inch gun under the usual conditions —viz., three rounds with 50 lbs. of powder and 260 lbs. chilled shell, at a distance of ten yards. The first shot produced the low indent of 3.7 inches without any crack, while the indents of the second and third rounds were 4.4 and 3.9 inches respectively. Cracks were produced by these shots, one extending to the edge of the plate. The charge of the 10-inch gun is 70 lbs., and the weight of the projectile 400 lbs. The range was the same as with the 9-inch gun. The first shot was fired at the right bottom corner, two feet from each edge, and produced a clearly defined indent of 4.4 inches, and several cracks circumferential to the point of impact. One of these reached to the bottom edge, and extended through the plate. The second shot was directed against the left bottom corner, 19 inches from the side and 23 inches from the lower edge, while the third fell at the right top corner, 19 inches from the top edge and two feet from the side. Owing to the points of the shell remaining fixed in the plate the depth of the indents could not be measured. The bulges at the back vary from ⅜ to ¼ in height, and have not opened out. Considering the severity of the second test, and that there was hardly room left for another shot, the damage effected was slight, and the plate would still have afforded efficient protection. The heavier gun seems to have slightly pushed in the entire surface of the plate within certain areas defined by various injuries, but without showing any increased penetration. In time the plate would have been cracked through and through and broken up under the severe cannonade; but it is clear that not a splinter would have found its way into the ship so protected. At present we know the effect of the 9, 10, and 12½-inch guns upon compound armor 11-inch plates, and experiments which are about to take place at Spezia will determine whether 19-inch plates can withstand the attack of the 100-ton chambered gun fired pointblank at short ranges. As this gun is considered capable of piercing iron armor over 13 inches in thickness, the results will be watched with the greatest interest. The comparative thickness of the steel-faced armor is an important factor in the trial. The targets to be fired at at Spezia will consist of two entirely steel plates, manufactured by Schneider at Creusot, and two compound armor plates by Messrs. Cannell and Sir John Brown & Co., of Sheffield. Their dimensions are 9 feet by 12 feet, the compound armor having steel surfaces one third of the whole thickness.

NEW IRONCLAD.—A new armorclad, for which the blocks have been some time in readiness, is about to be laid down forthwith at Portsmouth. She will be of the kind known as the "Admiral" type and may be regarded to some extent as an answering move on the part of the Admiralty to the gigantic shipbuilding projects of the Italian Government. While the *Rodney* and the *Howe* exhibit certain improvements upon the design of the *Collingwood*, the *Camperdown*, the name of the new ship, will in her turn display various modifications upon the design of the *Rodney* and *Howe*. She will differ from the latter in being 5 feet longer, having 400 tons greater displacement, and carrying stronger barbette armor. Her dimensions will be as follows:—Length, 330 feet; extreme breadth, 68 feet 6 inches; mean draught, 26 feet 9 inches; and displacement, 10,000 tons. She will be propelled by twin screws, the engines being contracted to develop with the use of forced draught 9,800 horses. It may be useful to contrast with these data the measurements of the *Duilio*, which are:—Length, 341 feet; breadth, 64 feet 9 inches; displacement, 10,434 tons; indicated horse-power, 7,500. While, therefore, the displacement of the English ship is slightly less than the *Duilio*, her engine-power is considerably greater, and is estimated to give her, in spite of her broader beam, a speed of 16 knots, or two knots an hour more than the Italian turret ship. She will be armored to the depth of five feet below the water-line and will be protected by a belt rising 2 feet 6 inches above the water-line. Her armor will consist of compound plates of the following thicknesses:—On the side, 18 inches; bulkheads, 16 inches, barbettes (normal), 14 inches and 12 inches; conning tower, 12 inches and 9 inches; and screw bulkheads, 6 inches. She will differ from all existing vessels, either armored or unarmored, in having vertical ventilating tubes extending from the flying deck to the lower deck. These tubes will be armored to the thickness of 12 inches. She will be also protected by an armored deck 3 inches thick over the belt and 2½ inches thick below the water-line at the ends, while the protection under the base of the barbettes will be three inches. Her armament is at present arranged to consist of four 63-ton B L.R. guns, and six 6-inch B.L R. guns, besides a complement of boat and machine guns and Whitehead torpedoes. Her bunkers are to hold 900 tons of coal, and her ship's company is intended to comprise 430 officers and men. The *Camperdown* will be a

sister ship of the *Benbow*, the contract for which has just been accepted by Messrs. Palmer Brothers, of the Tyne.

BOOK NOTICES.

LIGHT. By Lewis Wright, London: Macmillan & Co. Price $2.00.

This is a book for the experimenter and chiefly for the lecturer who employs the magic lantern.

Beginning with a description of the lantern and its accessories, the author then describes the common experiments illustrative of reflection, refraction, dispersion, color, spectrum analysis, phosphoresence, fluoresence, interference and polarization.

The work is illustrated with 190 woodcuts and 7 full-page plates.

Though many of the experiments are not as satisfactory as those by which they have been of late replaced in this country, the book will prove of considerable value to lecturers on physics.

GEOLOGICAL SKETCHES AT HOME AND ABROAD, by Archibald Geikie, LL.D., F.R.S. Price, $1.75.

The records of geological rambles by one of the foremost of living scientists possess a value to scientific readers apart from the literary character of the essays. The present collection, however, will be widely read by others than scientists or students, who will be fully repaid by the charming method by which the author imparts an interest in things usually passed by as uninteresting.

The key-note is struck in the first essay wherein the author, under the title of "My First Geological Excursion," describes his holiday rambles with his school-boy companions in search of limestone fossils. The enthusiasm awakened in those early days is manifested in his latest essays. They are still holiday rambles.

But when the reader is reminded that the writer is the highest living authority in matters relating to structural geology, and is, moreover, Director-General of the geological survey of the United Kingdom, he will regard the pleasant narrative as authoritative statements which will hereafter be counted as substantial additions to our present knowledge.

MUSICAL ACOUSTICS. By John Broadhouse. London: William Reeves. Price, $3.00

This work is designed particularly for students of music, but will prove to be profitable reading for students of physics.

Quotations from standard works are freely used by the author; Helmholtz, Tyndall, Pole and Sedley Taylor are each repeatedly drawn upon at considerable length.

The subjects of Consonance and Dissonance, Combination Tones, Consonant Chords, Scales and Temperaments, are treated with exceptional fulness for a hand-book.

The illustrations, more than one hundred in number, are good.

TUNNELING — EXPLOSIVE COMPOUNDS AND ROCK DRILLS. By Henry S. Drinker. Second edition, Revised and Enlarged New York: John Wiley & Sons. Price, $25.00.

The first edition of this work became widely known. It was published only four years since and the edition was exhausted a year ago.

The author has taken advantage of the opportunity to carefully revise the work and has made some important additions, relating chiefly to explosives, rock drills and air compressors.

Some valuable tables relating to drilling in the Sutro and St. Gothard Tunnels, and also some data relating to tunnels in India, will be found among the new matter.

MISCELLANEOUS.

THE following measurements of the great lakes of America have been taken by the Government surveyors:—The greatest length of Lake Superior is 335 miles; its greatest breadth is 160 miles; mean depth, 688 ft.; elevation, 627 ft.; area, 82,000 square miles. The greatest length of Lake Michigan is 300 miles; its greatest breadth, 108 miles; mean depth, 690 ft.; elevation, 506 ft.; area, 23,000 square miles. The greatest length of Lake Huron is 300 miles; its greatest breadth is 60 miles; mean depth, 600 ft.; elevation, 274 ft.; area, 20,000 square miles. The greatest length of Lake Erie is 250 miles; its breadth is 80 miles; its mean depth is 84 ft.; its elevation, 26 ft.; area, 6 000 square miles. The greatest length of Lake Ontario is 180 miles; its greatest breadth, 65 miles; its mean depth is 500 ft.; elevation, 261 ft.; area, 6,000 square miles. The total of all five is 1,265 miles, covering an area of upwards of 315,600 square miles.

DR. FLEISCHER, of Germany, describes a new system of hydraulic propulsion for ships. He dispenses with a turbine, and allows the steam to act directly upon the water in two large vertical cylinders placed amidships. These two cylinders communicate with the ejecting nozzles, which are situated on either side of the keel. In each cylinder there is a "float" or piston of nearly the same diameter as the cylinder, with a closed spherical top; when this float is in its extreme upper position, the cylinder is full of water. Steam is then admitted into the upper part of the cylinder above the float, the latter is pressed down, and the water is expelled through the nozzle-pipe with great velocity. At a certain portion of the stroke, the admission of steam is shut off automatically, the remainder of the stroke being performed during the expansion of the steam, and the velocity of ejection of the water gradually diminishing. At the conclusion of the stroke, the exhaust valve from the steam space to the condenser is opened, the steam rushing out, forming a partial vacuum above the float, and the water enters, pressing the float up.

A VALUABLE contribution to the subject of the electricity of flame has been lately made by Herren Elster and Geitel. The discrepancies in previous results are attributed

largely to the behavior of the air layer immediately outside of the flame having been left out of account. The authors used a Thomson quadrant electrometer for measurement. They find the supposed longitudinal polarization of flame merely apparent, and due to unequal insertion of the wires used as electrodes. On the other hand, flame is strongly polarized in cross section: an electrode in the air about the flame is always positive to one in the flame. The theory the authors adopt is this: By the process of combustion *per se* free electricity is not produced in the flame; but the flame-gases and the air-envelope have the property of exciting, like an electrolyte, metals or liquids in contact with them. To this electrolytic excitation is added a thermo-electric, due to the incandescent state of the electrodes. The amount and nature of the electric excitation is independent of the size of the flame, and dependent on the nature, surfaces condition, and glow of the electrodes, and on the nature, *Nature* says, of the burning gases. It is remarked that flames may be combined in series like galvanic elements, and so as to form a "flame battery."

ALLOY FOR SILVERING METALS.—A method for silvering, or, more properly, whitening metals, has been recently devised by M. de Villiers. It is a modification of the tinning process, an alloy being used instead of the pure tin. This alloy consists of 80 parts tin, 18 parts lead, and 2 parts silver, or 90 parts tin, 9 parts lead, and 1 part silver. The tin is melted first, and when the bath is of a brilliant white the lead is added in grains, and the mixture stirred with a stick of pinewood, the partially-melted silver is added, and the mixture stirred again. The fire is then increased for a little while, until the surface of the bath assumes a light yellow color, when it is thoroughly stirred up and the alloy cast in bars. The operation is then carried out in the following manner:—The article, a knife-blade for example, is dipped in a solution of hydrochloric or sulphuric acid, rinsed with clean water, dried and rubbed with a piece of soft leather or dry sponge, and finally exposed to a temperature of 70 deg. or 80 deg. Cent.—158 deg. to 176 deg. Fah.—for five minutes in a muffle, to prepare the iron or steel to receive the alloy, by making the surface porous If the iron is not very good the holes are large, and frequently flaws and bad places are disclosed, which make the silvering more difficult. With steel the process goes on very regularly. The article, warmed to say, 140 deg. Fah., is dipped in the bath, melted in a crucible over a gentle fire. The bath must be perfectly fluid, and is stirred with a stick of pine or poplar; the surface of the bath must have a fine white silver color. For a knife-blade an immersion of one or two minutes is sufficient to cover it; larger articles require five minutes of immersion. After taking it out of the bath it is dipped in cold water, or treated so as to temper it, if necessary. If left too long in cold water it frequently becomes brittle. It is then only necessary to rub it off dry and polish without heating it. Articles treated in this manner look like silver, and ring like it too, and withstand the oxidizing action of the air. To protect them from the effect of acid liquids like vinegar, they are dipped in a bath of amalgam, composed of 60 parts mercury, 39 parts of tin, and 1 part of silver; then dipped warm into melted silver, or electro-plated with silver to give them the silvery look. This kind of silvering is said to be very durable, and the cost comparatively small.

M. MEKARSKI, well known in connection with compressed air tramway engines, has published calculations to show that compressed air could not be used for long tunnels except at some difficulty. With a pressure of 5 kilogrammes per square millimeter, and an average temperature of 15 deg. C., the work of the compressed air, expanding two and a half times, would be 11,179 kilogrammeters, and the comsumption of air per hour per horse-power would be 24 15 kilogrammes. For one passage through the tunnel, the consumption of air at ordinary pressure would be 64,915 kilogrammes, or 177 cubic centimeters, at a pressure of 30 atmospheres. Placing the latter figure at 200 for safety's sake, and computing the weight of the reservoirs to carry the compressed air at 600 to 700 kilogrammes per cubic meter, we should have a total weight of the tender containing the necessary compressed air of 200 tons, which would reduce the load carried from 400 tons, as supposed in his calculations, to 200 tons. M. Mekarski proposes instead, to use the ordinary locomotives, and to run them with a mixture of air and steam. He carries the air in reservoirs—capacity 20 cubic meters—at a pressure of 35 kilogrammes per square inch. These reservoirs communicate with the boiler through an automatic device, which allows the air to enter it only when steam pressure falls below a given minimum. An auxiliary pipe from the air reservoir is to be conducted under the grate, in order to increase the rate of combustion if necessary. The engineer runs the locomotive with a growing quantity of air as he gets farther into the tunnel, and thus M. Mekarski thinks he could reduce the quantity of coal burnt in the tunnel.

IN a recent lecture on some of the dangerous properties of dusts, Professor Abel, F.R.S., said that many experiments were tried with sensitive coal-dust from Seaham and other collieries for the purpose of ascertaining whether results could be obtained supporting the view that coal-dust, in the complete absence of fire-damp, is susceptible of originating explosions and of carrying them on indefinitely, as suggested by some observers, but, although decided evidence was obtained that coal dust, when thickly suspended in the air, will be inflamed in the immediate vicinity of a large body of flame projected into it, and will sometimes carry on the flame to some small extent, no experimental results furnished by these experiments warranted the conclusion that a coal mine explosion could be originated and carried on to any considerable distance in the complete absence of fire-damp. Some experiments made in a large military gallery at Chatham

showed that the flame of a blown-out shot of 1½ lbs. or 2 lbs. of powder might extend to a maximum distance of 20 ft., while in a very narrow gallery, similar to a drift-way in a mine, the flame from corresponding charges extended to a maximum distance of 35 ft. These distances are considerably inferior to those which flame from blown-out shots has been known to extend, with destructive results, in coal mines, and there appears no doubt that, in the latter cases, of which the lecturer gave examples, the flame was enlarged and prolonged by the dust raised by the concussion of the explosion. But in the presence of only very small quantities of fire-damp, dust may establish and propagate violent explosions; and that, in the case of a fire damp explosion, the dust not only, in most instances, greatly aggravates the burning action and increases the quantity of after-damp, but that it may also, by being raised and swept along by the blast of an explosion, carry the fire into workings where no fire-damp exists, and thus add considerably to the magnitude of the disaster.

DR. BJERKNES has advanced beyond the results of his experiments shown at Paris. These were chiefly confined to illustrating the static attractions and repulsions of electricity and magnetism, but he has now taken up the subject of electro-dynamic attractions and repulsions. The former effects are shown by brass balls oscillating, or by small drums pulsating near each other in water. These motions are communicated to the balls and drums by pulses of air transmitted from an ingenious air-pump or bellows along india-rubber tubes. A pulsating drum corresponds to a magnetic pole; an oscillating body to a magnet. When two drums are vibrating near each other in like phase, they attract; when in unlike phase, they repel each other. The same holds true of the oscillating balls. The motion-lines round these bodies correspond to the lines of force round magnets, as was demonstrated by a hollow ball oscillating on a stem, and tracing its movements in ink on a glass plate. The more novel part of the experiment, *Nature* says, consisted in representing the attraction between two electric currents flowing in the same direction by means of two cylinders about 5 inches long and 1 inch in diameter, oscillating round their longitudinal axes at close quarters in the water. The cylinders were oscillated by means of a pulsating drum which communicated its motion to them by a toothed gearing on their ends. Attraction resulted when the oscillations of the cylinders were opposed to each other, and repulsion when they were in the same direction. A square of four oscillating cylinders was also formed, and a fifth cylinder oscillated inside it, the attraction or repulsion exerted on the latter being observed. A hydrodynamic galvanometer was made by placing an oscillating ball beside an oscillating cylinder, the result being a deflection of the ball according to the direction of the oscillation of the cylinder.

THE utilization of the earth's international heat is a subject which, *Nature* says, is attracting the attention of scientific men in Japan just now. At a recent meeting of the Seismological Society, Mr. Milne introduced the subject for the consideration of the members. He first drew attention to the fact that philosophers have told us the whole available energy upon the surface of the earth had in some way or other its action and its existence traceable to the sun. That there was an unlimited supply of energy in the interior of the earth was a circumstance which had, he said, been overlooked. In speaking of this energy, Mr. Milne first referred to that portion of it which crops out upon the surface in countries like Japan, Iceland and New Zealand, in the form of hot springs solfataras, volcanoes, &c. He stated that there was an unlimited supply of water in hot springs within a radius of 100 miles around Tokio, and that the heat of these springs could be converted into an electric current, and the energy transmitted to the town. The second part of the paper referred to the possibility of obtaining access to the heat which did not crop out in the surface.

THE pigments employed to color hydraulic and other cements, and obtain the shades common in trade, are, according to the *Bauzeitung*, the following, the proportions used being those used by R. Dyckerhoff, of Amoeneburg: For black, pyrolusite, 12 per cent.; for red, caput mortuum, 6 per cent.; for green, ultramarine green, 6 per cent.; for blue, ultramarine blue, 5 per cent.; for yellow and brown ochre, 6 per cent. The strength of the cement is rather increased by the addition of ultramarine pigments, but somewhat diminished by the others. The ill effects of the latter may be somewhat removed by grinding the cement again after the pigment has been added, whereby it gains in fineness, and the strength is so much increased that no difference is observable between this and the ordinary cement. The black and red cements made in Dyckerhoff's works for making tiles and artificial stone show a strength by normal tests after twenty-four hours' drying of 20 kilos. per square centimeter, or about 275 lbs. per square inch—a very respectable strain for such work.—*Engineer.*

THE MAGNAGHI FLOATING COMPASS.—The floating compass, invented by Captain Magnaghi, is now in use on board the Duilio, and will probably be generally adopted in the Italian Navy. Its main feature is the suspension of the needle in water, to which has been added one-tenth its volume of alcohol, contained in a vessel with a perforated bottom, which allows the liquid to rest ultimately on an elastic diaphragm. The addition of the alcohol prevents the water from freezing under low temperatures; and the elastic diaphragm allows it to expand and contract during atmospheric changes, without danger of breaking the glass which covers it, or admitting air. On this liquid the needle floats, enclosed in a heremetically-sealed ellipsoidal case, which is very delicately suspended upon a conical brass pivot. The pivot has a sapphire top and a jade point, and the friction is diminished to the utmost possible degree by the most perfect polish. The needle usually consists of six bundles,

each made up of five pieces of the best ribbon steel, thoroughly tempered before being magnetized, and separately tested after. These pieces are kept apart by strips of cardboard soaked in oil, and their number can be increased if necessary. Wherever in the apparatus two metal surfaces or edges meet, friction is prevented, and closure secured, by a layer of blotting paper soaked in mineral wax. This is exclusively used for the purpose, because it is insoluble in alcohol; and even the marks and figures in the outside ring are rendered distinct by being filled in with the same substance blackened. All the interior parts of the instrument are silvered, in order to prevent oxidization and galvanic action between the various metals composing it, and to keep the fluid perfectly colorless and transparent. The compass proper (including the floating case with the needles) weighs in the air about 750 grammes; but in the liquid it exercises a pressure of only about 6 grammes on the point of support. The chief advantage claimed for this invention is that the resistance of water being great towards rapid movements and inconsiderable towards slight ones, it leaves the motions of the needle practically free, while shielding it (by its own incompressibility) from all shocks from without. The compasses of the Duilio were not in the least agitated by the discharge of the 100-ton gun, nor by the motion of the screw, although the supports on which they were placed were in such a position as to feel the vibration greatly. They were somewhat disturbed by the rolling and pitching of the vessel; and to meet this difficulty, modifications were made in the shape and arrangement of the different parts, so as to render the floating case thoroughly centrifugal, distribute great portion of the weight round the circumference, and fix the point of suspension very little above the center of gravity. The result of these arrangements is, that when the compass is tilted by the movement of the ship, the needle is so slow to change its position, that before it has again become horizontal the motion is reversed, and the inclination counteracted. The needle is also very little affected by changes in the angle at which the terrestrial magnetic current is inclined to the horizon, which varies in different localities, in consequence of the needles being so much shorter than the diameter of the compass, and being placed too low with regard to the point of suspension. This is proved by the simple test of holding a powerful magnet directly over the north point of the compass, when even this great increase to the vertical force produces only a very slight change in the inclination of the needle. The compass is fitted with a special sextant, in which various improvements have been introduced, to increase the facility and accuracy with which observations can be taken, especially in twilight and cloudy weather. A detailed description of both instruments, with illustrations, will be found in the *Rivista Marittima* for February and April.

A SOFT alloy which will adhere so firmly to metallic, glass, and porcelain surfaces that it can be used as a solder, and which is invaluable when the articles to be soldered are of such a nature that they cannot bear a high degree of temperature, consists of finely pulverized copper or copper dust, and is obtained by resolving copper sulphate, or vitriol of copper, into its original elements, by means of metallic zinc. Twenty, thirty, or thirty-six parts of this copper dust, according to the hardness desired, are placed in a cast iron or porcelain-lined mortar, and well mixed with some sulphuric acid having a specific gravity of 1.85 Add to the paste thus formed 70 parts (by weight) of mercury, constantly stirring. When thoroughly mixed the amalgam must be carefully rinsed in warm water to remove the acid, and then laid aside to cool. In ten or twelve hours it will be hard enough to scratch tin. When it is to be used it should be heated to a temperature of 375 degrees C.; when it becomes as soft as wax by kneading it in an iron mortar. In this ductile state, the *Scientific American* says, it can be spread upon any surface, to which, as it cools and hardens, it adheres very tenaciously.

IT is stated that a new lamp combining gas and electricity, giving remarkably economical results, has been brought out. It will be remembered that some years ago gas burners were not uncommon which had a small piece of platinum foil arranged on the burner so as to be burned in the flame. When this was heated by a gas flame, it, by a regenerative action, heated the gas coming from the burner, and caused an improvement in the light. The new lamp is essentially, it is stated, this burner arranged so that a small current of electricity is passed through the platinum. The gas is first lighted, and this heats the platinum, the resistance of which is thus increased, so that a current which would when the platinum is cold, be freely transmitted, now heats the platinum to incandescence, and thus in turn heats the issuing gas to a very high termperature, so that a light equal to 30 candles is, it is said, obtained by the consumption of 2 cubic feet of gas per hour, and a small electric current. If this is the case, the existing gas fittings are all utilizable, and a secondary battery of no great number of elements, and charged with a current of about 2½ volts E.M.F., would supply the current needed.

TUNNEL VENTILATION.—A "chemical lung" is the latest thing proposed for the ventilation of tunnels. It was lately tested in London by fourteen scientists. A room 15′ x 18′ was kept for an hour at a temperature of 82 degrees, and the air was loaded with impurities. The men of science were now called upon to enter, and the air was made still more impure by burning sulphur and carbonic acid gas. Then the "chemical lung," or punkah, so called, measuring 4′ x 2′ 6″, was set in motion. The temperature was soon reduced to 65 degrees, and the air freed from all impurities. Then fat was burned, to test the machine for organic substances, and the "lung" was started up just in time to prevent the examining gentlemen from running out for fresh air. It is proposed to use the invention during the construction of the channel tunnel.